CASH TRANSFERS, BEHAVIORAL CHANGE, AND CHILD DEVELOPMENT: EXPERIMENTAL EVIDENCE FROM MALAWI'S CASH TRANSFER PROGRAM

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ABSTRACT

Kelly Kilburn: Cash Transfers, Behavioral Change, And Child Development: Experimental Evidence From Malawi's Cash Transfer Program
(Under the direction of Sudhanshu Handa)

This dissertation examines the impacts of a social cash transfer program for poverty alleviation in Malawi on caregiver and child outcomes and whether these impacts can be explained by changes to household behaviors or mindsets. The data for this study comes from a cluster-randomized study of the Government of Malawi's Social Cash Transfer Program that provides unconditional cash payments to ultra-poor, labor-constrained households. After a baseline survey, households in the study were randomly chosen to the treatment group to receive transfers immediately or to the later entry control group. A follow-up survey was then conducted after 12 months of transfers to the treatment group. This dissertation uses this experimental panel data to provide causal evidence on whether Malawi's program impacts child outcomes and parental behaviors. The goal of this research is to come to a greater understanding of how cash transfers may improve the chances of ending poverty transfers across generations. This dissertation consists of three essays to meet this goal: In the first essay, I show the impacts of the cash transfer program on adult caregiver subjective well-being. In the second and third essays I examine child development outcomes of schooling and mental health respectively and whether parental behaviors mediate the direct impact from the cash transfer.

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CHAPTER 1: INTRODUCTION

Poverty and vulnerability are two interlinked conditions that contribute to poverty traps across generations. Poverty makes people vulnerable to conditions such as ill health, unemployment, and shocks like conflict or famine while vulnerability enhances poverty, limiting people's ability to rise out of poverty (Philip & Rayhan, 2004). The severity of these conditions in the developing world means that families in chronic poverty suffer cumulative and enduring consequences, which is transmitted down to their children (Barrientos & Dejong, 2006). Children who grow up in poverty can suffer detrimental impacts to their health, human capital, and psychosocial well-being (Case, Paxton, & Ableidinger, 2003; Harper, Marcus, & Moore, 2003). These factors contribute to the persistence of poverty as children transition into their adult lives, reinforcing the poverty cycle as they have their own children (Barrientos & Dejong, 2006).

Knowledge is needed about the ways existing policies address the widespread poverty and vulnerability of these children and families to find what really works to end the poverty cycle. There are a multitude of causal factors at play in intergenerational poverty transmission including economic, political, social, and environmental ones, but one of the main challenges for policy-makers is to understand how to effect change at the micro or individual level because individual-level behaviors strongly influence the cycle of intergenerational poverty (Harper et al., 2003).

In sub-Saharan Africa (SSA), the vulnerability of the family has become more pronounced as mounting traditional drivers of poverty have interacted with the rise of the AIDS epidemic (Samson, 2009). Weakening family systems have become less able to provide financial protection and other forms of support, resulting in the increase of poverty and instability, especially for children (Kaseke, 1996; Adato & Bassett, 2008).

Efforts to respond to these issues have led to promotion of social protection policies to create enabling environments for family unification and support. According to the Organization for Economic Co-operation and Development, social protection policy focuses on "uplifting the poor and vulnerable by enhancing their capacity to cope with poverty, equipping them to better manage risks and shocks" (Samson, 2009). Social cash transfers in particular, have emerged in the developing world as a lead social protection initiative. They support the goal of social protection by giving consistent, non-contributory cash payments to the most poor and vulnerable in society.

Evidence shows that cash transfers have several mechanisms to enhance the livelihoods of the poor and vulnerable. First, the consistent, extra cash payment becomes a stabilizing force for the household, helping them to purchase essential food items and health related expenses (Handa, Devereux, & Webb, 2010). Second, they help to mitigate the risks of living in poverty, protecting households from shocks and encouraging productive investments (Samson, 2009). Third, cash transfers have a transformational component by supporting human capital investments in children, which can help households overcome intergenerational poverty (Handa et al., 2010).

In SSA, there is a focus on unconditional cash transfers that distribute payments regardless of recipient behaviors as opposed to conditional transfers that tie receipt to certain conditions. Unconditional cash transfers are seen as an alternative way to promote behavior change because they give direct economic power to the poor by letting them make the decisions on how to spend the money (Handa et al., 2010). Additionally, cash transfers in Africa are most often targeted to household units and thus have an important job in revitalizing the family and its system of traditional support. Families play a decisive role in so many aspects of individuals' lives as they provide the daily environment where individuals eat, sleep, communicate, and learn social skills. Since families provide this critical environment, policies targeted to them can affect the micro-level behavior change that can have the biggest and most efficient impact on poverty alleviation (Arriagada, 2011).

Africa has already invested heavily in social cash transfers and with plans to expand programs,

millions of people will eventually be reached (Samson, 2009). The growing scale of social cash transfers means that there is a need for comprehensive evaluations to understand the breadth and depth of impacts to inform policymakers and enhance literature on social protection policy. While previous evaluations have provided extensive positive evidence that unconditional cash transfers can improve family and child welfare and empower better decision-making (productivity—Covarubbias, Davis, & Winters, 2012; schooling—Kenya CT-OVC, 2012; Miller & Tsoka, 2012; Robertson et al., 2013; sexual behavior of youth—Baird, Chirwa, McIntosh, & Özler, 2010 ; Handa et al., 2014), the literature is lacking evidence on the processes that change behavior.

Most evaluations of poverty alleviation programs have focused on the important work of predicting relationships between independent and dependent variables, essential for making claims about policy effectiveness. However, another important objective in social science research is to explain these relationships to improve our understanding of the world, and for policy analysts, to enhance policy design. Therefore, explaining the relationships between SCT receipt and behavior change is important to gain knowledge of why we observe certain impacts and will help inform better policy design and targeting. Moreover, understanding processes is particularly important in the case of unconditional cash transfers since there are no conditions to attribute changes in behavior.

This dissertation evaluates a social cash transfer program in Malawi to understand how it affects family and child well-being and transforms behaviors known to keep families in poverty. The data for this study comes from a cluster-randomized study of the Government of Malawi's Social Cash Transfer Program. Baseline survey data was collected in the summer of 2013 and then households were randomly assigned to either the treatment group (T) to receive transfers immediately or to the later entry control group (C). A follow-up survey was then conducted after 12 months of transfers in late 2014.

The goal of this proposed research is to come to a greater understanding of how cash transfers can impact individual and family behavior to enhance the prospects of ending intergenerational poverty cycles. This dissertation consists of three essays to address this goal:

Essay One

This essay analyzes the impact of Malawi's Social Cash Transfer Program on caregivers' subjective well-being using panel data from 3,365 households. Utilizing econometric analysis and panel data methods, we find that household income increases from the cash transfer can have substantial subjective well-being gains among caregivers. Households use the cash to improve their families' livelihoods, ensuring provision of their basics needs including food, shelter, and clothing. Reduction of these daily stresses makes caregivers happy about their current situations and gives them hope that the future will continue to get better.

Essay Two

This essay analyzes the impact of Malawi's Social Cash Transfer Program on child schooling outcomes including enrollment, dropouts, and withdrawals. We also examine potential intervening pathways that lie on the causal pathway between cash transfers and child schooling since households receive the cash and parents are responsible for making spending decisions. We use a differences-in-differences specification to test treatment impacts on a two-wave panel of 6,303 school-aged children (6-17). Findings indicate that the cash transfer program has strong schooling impacts; it improves enrollment rates and decreases dropouts. We also find that the main intervening pathway between the program and schooling is through education expenditures implying that the cash helps improve the demand for education by reducing financial constraints.

Essay Three

This study analyzes the impact of Malawi's Social Cash Transfer Program on adolescent mental health. In addition, we assess potential intervening pathways that lie on the causal pathway between household cash transfers and adolescent mental health since parents are responsible for spending the cash and because there are no conditions for how to use the money. We use a two-wave panel of 3,369 households and 1,332 adolescents. Results from our differences-in-differences model show no significant

effects of the cash transfer on depression for the full sample. However, for females and orphans known to be more at risk of depression, there is a significant reduction of depression in the treatment arm. While there are strong impacts from the cash transfer on potential intervening pathways, we do not find that these pathways explain much of the treatment effect on adolescent mental health.

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CHAPTER 2: HAPPINESS AND ALLEVIATION OF INCOME POVERTY: IMPACTS OF AN UNCONDITIONAL CASH TRANSFER PROGRAM ON WELLBEING USING A SUBJECTIVE APPROACH

Introduction

The importance of income for individual subjective well-being, often described as "happiness" or "life satisfaction", has been debated for decades (see for example: Easterlin, 1974; Fritjters, Haisken-DeNew, & Shields, 2004; Stevenson & Wolfers, 2008; Easterlin et al., 2010). Most evidence comes from examining correlations between average subjective well-being and national income in cross-sectional data. In richer, more developed countries income is positively correlated with happiness but with diminishing returns (Frey & Stutzer, 2002). At lower levels of income and in low-income countries however, there is a stronger linear relationship implying that income is an important determinant of happiness when it corresponds to a better quality of life in the sense of satisfying basic needs (Deaton, 2008; Graham & Behrman, 2009). The relationship between low-income and happiness suggests that poverty alleviation programs that have direct impacts on income may have the potential to increase subjective well-being. Nevertheless, little is known about the subjective well-being impacts of income increases for the poor, especially by means of specific policy.

This paper revisits the relationship between income and happiness and estimates the impact of an unconditional social cash transfer program on individual subjective well-being. Social cash transfer programs provide consistent cash payments to targeted, poor households. Unconditional cash transfers distribute payments regardless of recipient behaviors as opposed to conditional transfers such as those in Latin America that tie receipt to certain conditions. Limited evidence exists on the relationship between cash transfers and happiness, but a study in Mexico found a dissonance on objective and subjective

welfare; the reduction of income poverty for households in the Mexican Oportunidades program did not translate into a greater sense of well-being (Rojas, 2008).

While an interesting relationship to explore on its own, there is increasing attention in the measurement and use of subjective well-being as a means of informing policy design (Dolan & Peasgood, 2008). Mounting evidence has shown that subjective well-being metrics can capture individual emotional states and predict other measures of individual well-being such as health outcomes (Kahneman & Kruger, 2006). They also have the potential to predict behavior across other domains such as work life and relationships (Lyubomirsky, King, & Diener, 2005). Incorporating subjective welfare indicators into social policy evaluations can thus complement existing objective measures and provide a deeper understanding of how policies affect livelihoods across more dimensions than the economic one.

This paper uses data from an experimental study of Malawi's Social Cash Transfer Program to explore measures of subjective well-being that capture concepts of life satisfaction, relative wealth, and future expectations amongst household caregivers. The study collected longitudinal household data with a baseline survey in 2013 and a 17-month follow-up survey at the end of 2014. The households for this study were randomly assigned to either the treatment and control group after the baseline survey. While income is generally taken to be endogenous to both individual well-being and determinants such as health status and personality, the random assignment to treatment provides exogenous variation in income that allows us to identify the impact of such increases on subjective well-being. As a starting point, our empirical specification models subjective well-being traditionally as an additive function of individual determinants. We then use panel data methods to control for time trends and unobserved individual characteristics to elicit a casual impact of income.

This investigation finds that household income increases from the cash transfer can have substantial subjective well-being gains among caregivers. We find large, significant effects of treatment on both life satisfaction and future outlooks, which are robust across empirical specifications and additional controls. Specifically, after about a year's worth of transfers, caregivers in beneficiary households score 0.50 SD

higher on the Quality of Life scale and are over 20 percentage points more likely to believe in better future in two years using panel data specifications.

Background

Subjective well-being is defined by Diener, Lucas, and Oishi (2009) as an individual's evaluation of his or her life from both emotional and cognitive perspectives. Therefore, high subjective well-being can include the recurrent experience of positive affect (and low negative affect) as well as high life satisfaction. In practice, surveys such as the Gallup World Poll tend to capture more of the cognitive aspect of subjective well-being with questions about how an individual assesses their quality of life. Economists have traditionally been critical of these quality of life measures though because self-reports are assumed to be unreliable signals for individuals' underlying preferences and constraints that affect actual behavior. Instead, they have relied on revealed preference analysis, examining observable consumption and investment behavior with the underlying assumption that these measurable choices better reflect the set of unobservable trade-offs of preferences and constraints (Graham & Behrman, 2009).

Regularly, however, people's choices are not aligned with their own happiness. Literature from behavioral economics and psychology finds that people habitually make inconsistent choices, departing from the standard model of the rational economic agent (Kahneman, 2003). Evidence from developing country contexts finds that people repeat the same mistakes, fail to participate in market opportunities, and make myopic decisions that fail to take account of their long-term welfare such borrowing at very high interest rates (Anderson & Stamoulis, 2006; Banerjee & Duflo, 2004, Banerjee & Mullainthan, 2010). Revealed preference analysis is therefore limited in providing explanation of other factors influencing important choices, such as self-control problems and constraints like poverty that might result in perverse choices (Graham & Behrman, 2009). Moreover, personal psychological states have been clearly linked to individual economic and social behaviors. Decision-makers with positive life outlooks are expected to make better choices for themselves and their household such as seeking preventive care or

investing in human capital because happiness increases decision-makers' cognitive flexibility and self-control to carefully assess their future (Isen 2008; Lyubomirsky, King, & Diener, 2005).

In theory, collecting subjective data allows researchers to test fundamental economic assumptions because subjective data directly captures well-being (Frey & Stutzer, 2002). While the assumption that income is a basic determinant of subjective well-being has been tested and affirmed, supporters of the subjective approach do not condone using income to exclusively evaluate welfare for the risk of overvaluing policy impacts. There are other human needs and values that cannot be directly bought or enriched with income such as emotional support and personal relationships as well as autonomy and human development. Moreover, focusing solely on income neglects the fact that income may not be used efficiently and that well-being could depend more on relative rather than absolute consumption (Rojas, 2007). Alleviation of income poverty might not be enough to increase individual's overall sense of well-being if other dimensions of their life are going poorly. As Rojas (2009, 2015) describes in his 'subjective well-being approach', the goals of poverty alleviation programs may be compromised if dissonances emerge between subjective and objective measures. Policies that cannot improve people's lives across more dimensions than absolute income may not lead to successful transitions out of intergenerational poverty since well-being involves other aspects such as work, relationships, and communities. Thus, including subjective well-being measures within evaluations of social programs can complement objective measures to provide a better picture on the effect of policies across more dimensions than the economic one.

Though subjective well-being analysis may be useful in policy evaluation, it is important to point out that individuals' perception of well-being are not easily comparable between people. Even if subjective questions can appropriately capture individual well-being, researchers expect that individuals have different interpretations of subjective questions, which will bias interpersonal comparisons (Beegle, Himelein, & Ravallion, 2012). Nonetheless, econometric techniques that control for unobserved heterogeneity among individual responses makes the use of subjective survey data more acceptable for

policy analysis (Graham & Behrman, 2009). Moreover, recent findings indicate that differences in individual viewpoints present little bias in relative well-being data (Beegle et al., 2012), and measures of life satisfaction have been validated as a good correlate across other measures of well-being including economic, psychological, physiological ones (Dolan, Peasgood, & White, 2008; Kahneman & Kruger, 2006).

Therefore, subjective well-being measures have become increasingly popular among economists and policy analysts for the purpose of measuring individual and social welfare (e.g. Kahneman & Kruger, 2006; Mullainathan, 2005; Rojas, 2008; Rojas, 2009; Fafchamps & Shilpi, 2009; Di Tella & MacCulloch; 2006). However, despite the growing use and acceptance subjective well-being data, literature on well-being and income rarely makes causal claims because survey data is usually missing the exact timing of changes in income and happiness, raising concerns about reverse causality. A few studies, however, have been able to test this causality by utilizing natural exogenous variations in income. For instance, Frijters, Haisken-DeNew, and Shields (2004) use the reunification of Germany to show that income gains for East Germans resulted in lasting gains on individual life satisfaction. Gardner and Oswald (2007) use data on lottery winners in Britain, to show that mid-size wins result in better psychosocial health for winners compared both to those with no wins or smaller wins. Experimental data, however, is missing because as Gardner and Oswald (2007) point out "…it is not possible to run giant experiments where, in the name of science, different amounts of government-funded research cash are randomly allocated to treatment and control groups (p 50)."

This paper fills this important gap by exploiting the randomized study design to measure the causal impact of income increases on subjective well-being. Evaluations of unconditional cash transfer programs have just recently started to measure subjective well-being. In Kenya, Give Directly, an NGO that gives one-time unconditional cash payments to poor households, found that the program had strong positive effects on happiness and life satisfaction measures (Haushofer & Shapiro, 2013), however, there were larger negative spillover effects for neighbors that did not receive the transfer (Haushofer, Reisinger, &

Shapiro, 2015). Another working paper by Handa and company (2014) also uses subjective data from Kenya. The paper examines the country's large-scale government-run unconditional program (CT-OVC) and shows that subjective measures performed well and correlated to expected material well-being and demographic measures. The evidence from Kenya's CT-OVC program, however, is limited to cross-section analysis. This study uses longitudinal data from Malawi and thus improves upon this evidence by controlling for ex-ante well-being and any unobserved individual heterogeneity in survey responses. Additionally, comprehensive survey data allows us to control for individual and household determinants of subjective well-being including baseline consumption.

Study Setting and Design

Location

Malawi is a small, landlocked country in southern Africa. The majority of the nation's population (51 percent of 16.7 million) lived below the poverty line in 2014 and the vast majority of the population reside in rural areas, living as subsistence farmers. The country is one of the poorest in Africa; Malawi's 2014 GNI per capita figure of $790 (PPP, current international $) is less than 25 percent of the SSA average of $3382 (World Bank, 2014). According to the same data source, gross enrollment in secondary school was a low 37 percent in 2013, and unemployment was high at 14.8 percent for women and 12.6 percent for men aged 15-24 years in 2014.

The Malawi SCTP program

The Government of Malawi's (GoM's) Social Cash Transfer Program (SCTP) is an unconditional cash transfer program targeted to ultra-poor, labor constrained households in Malawi. The main objectives of the program are to alleviate hunger and poverty among households and to improve children's well-being and human capital through education, nutrition, health, and household productivity. The program began as a pilot in the Mchinji district in 2006 and since that time, the program has expanded to 18 districts and reached approximately 175,000 as of January 2016.

SCTP beneficiary selection is made through a community-based approach with oversight provided by local and national government. Appointed community members are responsible for identifying households that meet the eligibility criteria of being ultra-poor and labor constrained. After further screening of identified households by the GoM, including a proxy means test to meet the ultra-poor eligibility condition, the recipient list is generated. The program's goal is that these lists target the bottom 10 percent of each community (Malawi SCTP Evaluation Team, 2015). An early evaluation of the Malawi SCTP in Mchinji confirms that recipient households live in extreme poverty and have higher dependency ratios than other poor households (Miller, Tsoka, & Reichert, 2010). Additionally, household heads tend to be older (above 60) and upwards of 80 percent of households are missing at least one prime-age adult, highlighting their particular vulnerability to the impacts of HIV/AIDS (Handa et al., 2013).

The SCTP provides a monthly unconditional cash transfer to eligible households, which vary depending upon the number and school status of members in the household. Table 2.1 shows transfer amounts in Malawi Kwacha (MWK) that were in use at time of follow-up data collection (first column, 'Prior to May 2015') and the new transfer levels that were increased in May 2015.

Table 2.1. Structure and level of transfers (current MWK)

	Prior to May 2015	After May 2015
1 Member	1,000	1,700
2 Members	1,500	2,200
3 Members	1,950	2,900
4+ Members	2,400	3,700
Each primary school child[1]	300	500
Each secondary school member[2]	600	1,000

[1]Provided for household residents age 21 or below in primary school. [2] Provided for household residents age 30 or below in secondary.
Source: Malawi Social Cash Transfer Program Midline Impact Evaluation Report (2015)

The 'rule of thumb' among policy experts is that size of the transfer should amount to at least 20 percent of baseline consumption in order to have measurable impacts (Davis & Handa, 2015). Before the increase in transfer amounts in May 2015, the majority of beneficiaries in this study's sample had a share below this 20 percent threshold. The average share was 18 percent and 50 percent of beneficiaries had a share below 15 percent.

Study Design

Data comes from the impact evaluation of Malawi's SCTP undertaken by UNC-Chapel Hill's Carolina Population Center and University of Malawi's Center for Social Research. The design consists of a cluster-randomized longitudinal study with a baseline survey and two follow-up surveys. This study only uses the baseline survey conducted mid-2013 and the first follow-up survey conducted in late 2014 through early 2015. The household survey is the main survey instrument covering a comprehensive list of topics including household composition, consumption, economic activity, education, health, time use, and subjective welfare among others. A qualitative component also includes in-depth individual interviews with the caregiver and one youth from 16 treatment households selected using a stratified sampling approach. This study has IRB approval from both the University of North Carolina (IRB Study No. 14-1933) and Malawi's National Commission for Science and Technology (IRB Study No. RTT/2/20).

The evaluation was designed around the GoM's plans to extend and expand coverage of the SCTP within in Malawi over three years starting in 2013. In order to integrate the impact evaluation with early expansion plans in 2013, two districts, Salima and Mangochi, were chosen for this study. Random selection was included at all possible levels, including the two smaller levels within these districts, Traditional Authorities (TAs) and Village Clusters (VCs). First, two TAs in each district were first randomly selected to participate in the evaluation study and then eligible beneficiary lists from each VC within these four TAs were generated following normal program operating guidelines described above. In the second stage, VCs were randomly selected to arrive at a necessary sample size of 3,500 based on power calculations for key program outcomes. In Salima, all eligible households were selected in the VCs. In Mangochi, 125 eligible households per VC were randomly selected in each selected VC. The final sample for the study was drawn from 29 VCs and comprises 3,531 households, approximately 47 percent of all eligible households from the four TAs.

The quantitative baseline survey was administered over several months from June to September 2013 to the study sample of 3,531 households (1,678 treatment and 1,853 control). Households were not

assigned to treatment (T) and control (C) status until after the baseline survey in order to maintain objectivity during data collection. Half of the VCs in each TA were randomly assigned the treatment group to start receiving the cash transfer right away. The other half of the VCs was assigned to the delayed-entry control group and entered the program in late 2015. This cluster randomization approach is preferable to household randomization in this study because it reduces concerns that treatment effects could become contaminated due to households living in close proximity with other study participants (Malawi SCTP Evaluation Team, 2013). The design is also more administratively and ethically feasible because the program did not have the financial resources to reach all households immediately.

The follow-up occurred at the end of 2014 and concluded in February 2015. Overall attrition was low, 95 percent of the baseline sample was retained and detailed attrition analysis finds no evidence of selective attrition. Beneficiary households had received five or six cash payments at the time of follow-up data collection. Each payment accounted for two months so results can be interpreted as one-year impacts of the program (Malawi SCTP Evaluation Team, 2015).

An important note about the follow-up is that this survey was conducted in Malawi's lean season while baseline was conducted after the harvest. There was a significant decline in consumption of around 25 percent for both study arms at follow-up, on par with regional consumption fluctuations between the same time periods in Malawi's 2010 Integrated Household Survey. However, the SCTP appears to be protective for beneficiary households during these seasonal changes as evidenced by greater average consumption across a number of food and nonfood categories (Malawi SCTP Evaluation Team, 2015).

Data

For our purposes in this paper, the sample includes households who responded to subjective well-being questions in both waves. There was one respondent per household, typically the main caregiver in the household but not necessarily the household head. The full household panel includes 3,365 households (1,605 treatment and 1,760 control) consisting of all households that responded to subjective well-being questions in both waves. The individual panel of 2,919 (1,520 treatment and 1,399 control)

consists of the same respondent in both waves and is a smaller subset of these households.

Measures

To measure subjective well-being this study includes constructs of quality of life, relative well-being, and future expectations. Quality of life measures are constructed from a series of questions gauging individual's perceptions of life satisfaction (Douthitt, MacDonald, & Mullis, 1992). Life satisfaction refers to a person's global assessment of their life such as whether they find life pleasant or fulfilling. This is considered a cognitive, judgmental process, where a person's judgments are dependent upon a comparison of one's present circumstances with a standard which each individual sets for him or herself (Diener, Emmons, Larsen, & Griffin, 1985). Therefore, we did not externally impose any reference for comparison so that subjective well-being measures center on a person's own judgments.

To measure the quality of life, respondents were asked how much they agree with the following statements from strongly agree (5) to strongly disagree (1):

1. In most ways my life is close to ideal.
2. The conditions in my life are excellent.
3. I am satisfied with my life.
4. So far I have gotten the important things I want in life.
5. If I could live my life over, I would change almost nothing.
6. I feel positive about my future.
7. I generally feel happy.
8. I am satisfied with my health.

These questions are drawn from the Satisfaction with Life Scale (SWLS) (Diener et al., 1985) and the WHO Quality of Life Scale (WHOQOLS)(WHO, 1998). The first five questions comprise the SWLS, which is narrowly focused on an individual's overall life satisfaction. The SWLS has shown good internal consistency and construct validity (Kobau, Sniezek, Zack, Lucas, & Burns, 2010). The last three questions come from the WHOQOLS and covers positive affect as well as overall quality of life. Quality

of Life (QoL) is a continuous measure generated by summing the responses from each item of the scale. The resulting scale ranges from 8 to 40 with higher scores reflecting greater quality of life.

The study also collects caregiver perceptions of their relative well-being with respect to their societal economic status. Literature has confirmed that income evaluated relative to others has a significant effect on individuals' perception of well-being at least among developed societies (Clark et al., 2008). Evidence from developing societies is more inconsistent. For example, Ravallion and Lokshin (2010) find that among the poor in Malawi, subjective well-being is not correlated with mean income in one's neighborhood. However, Fafchamps and Shilpi (2009) find that relative consumption is an important predictor of subjective well-being among the poor even in isolated villages in Nepal.

We measure relative well-being using a visual stepladder with six choices from poor (1) to rich (6). Respondents are asked to place themselves on one of these steps in addition to their neighbors and friends. We generated two binary variables, one that measures relative well-being in comparison to friends and the other in comparison to neighbors. The variables measure if individuals perceive themselves to be either the same or better off compared to worse off than their friends and neighbors.

The last construct, future outlooks, is measured by asking respondents for their perception of how they feel their life will go (better, same, or worse) in one, two, and three years from now. Binary indicators measure whether individuals feel their life will be better off in the future compared to the same or worse off. As compared to life satisfaction, which is an assessment of one's current circumstances, these questions on future well-being have respondents gauge the unknown future and tap into concepts of expectation and optimism. Psychological theory proposes that optimism as a personality trait would affect subjective well-being through expectations about the future (Scheier & Carver 1985). Some literature has found that dispositional optimism correlates well with other measures of subjective well-being such as life satisfaction and positive affect (Lucas Diener, & Suh, 1996). Optimism may also be a latent sentiment too. Experimental evidence from an intervention targeting gratitude show that participants exposed to a self-guided reflection of their blessings targeting could cultivate optimism about the near future (Emmons

& McCullough, 2003).

The independent variable is treatment status, a binary measure for households randomly chosen to receive the cash transfer. Individual correlates of subjective well-being controlled for include age, age-squared, gender, marital status, education, and chronic health issues (Dolan et al., 2008; Wiemann et al., 2015). Baseline values of household correlates are also controlled for including household size and total members in age groups (0-5, 6-11, 12-17, 18-65, and 65+). Baseline values of these measures are used because household composition could be endogenous to the income shock.

If a positive income shock increases happiness, shocks that would reduce income such as death of an income earner should analogously have a negative impact on happiness. By testing the relationship between negative shocks and subjective well-being, we can ensure that our measures are sensitive to negative shocks and respond appropriately. Respondents were asked about negative shocks that occurred within the previous 12 months such as floods and droughts, high food prices, death and serious illness of household members, and conflict or violence. We test a couple of measures, total number of shocks and an indicator for the death of an income earner. In addition, respondents were asked to assess the likelihood of experiencing negative shocks in the next year, a food shortage and needing financial assistance. Indicators for each shock measure whether the respondent believes there a likely or very likely chance the event will occur in the next year.

Methods

The determinants of subjective well-being (SWB) are typically modeled empirically as an additive function of the social, economic, and environmental factors (Xs) involved where the error term (ε_i) captures individual differences in reporting (Dolan et al., 2008). :

$$SWB_{it} = \alpha + \beta_1 X_{1_{it}} + \beta_2 X_{2_{it}} + \cdots + \varepsilon_{it}$$

We start with this basic specification and use three different models to test the effect of treatment on SWB. First is an OLS linear regression model on the wave 2, cross-section data.

$$(1) \quad Y_i = \alpha + \beta_1 T_i + \beta_2 X_i + e_i$$

Y_i is the individual measure of subjective well-being, T_i is an indicator variable for being in a treatment household, and X_i is a vector of individual control variables.

The second model is a Differences-in-Differences (DD) regression model, which uses panel data to account for baseline values of SWB and group level differences across the two study arms. Equation 2 shows the basic empirical specification where Y_{it} is an individual, time specific measure of subjective well-being, T_i*P_t is an indicator for cash transfer receipt in the second wave and represents the DD estimate of the treatment effect since it is the product of treatment status (T_i) and second time period (P_t).

$$(2)\ Y_{it} = \alpha + \beta_1 T_i * P_t + \beta_2 T_i + \beta_3 P_t + \beta_4 X_{it} + e_{it}$$

The final specification is a fixed effects model to control for individual reporting differences and unobserved characteristics such as personality that might bias the treatment effects. These show up in the unobserved error term, V_i, and are assumed fixed over time. Individual fixed effects will also wipe out any time indifferent control variables such as gender and treatment status.

$$(3)\ Y_{it} = \alpha_i + \beta_1 T_i * P_t + \beta_2 P_{it} + \beta_3 X_{it} + e_{it} + v_i$$

Regressions include household probability weights and standard errors are clustered at the village cluster level, the level of random assignment to treatment.

Results

Randomization and Summary Statistics

The data for this study comes from a carefully designed, randomized experiment and thus in theory, second round results should be enough to find a treatment effect if randomization was successful at balancing T and C groups. Nonetheless, in a field experiment that is part of a larger governmental intervention, successful randomization is more difficult to achieve than experiments designed and implemented by the same research team. For example, we might find bureaucrats affecting randomization (intentionally or not), resulting in imperfect implementation. Randomization may also be imperfect and where randomization results in unevenness between groups, it is important to control for those baseline characteristics. Bruhn & McKenzie (2009) even recommend controlling for baseline variables that are

thought to influence future outcomes, whether or not their means are statistically different, because including variables that are good predictors of the outcome soak up residual variance, increasing power. Additionally, examining balance in this particular evaluation is important because the randomization of the program was done at the cluster level, while the outcomes of interest are at the individual level.

Table 2.2. Success of randomization: key indicators at baseline by treatment status

	Baseline characteristics	
	Treatment	**Control**
Observations	*1,608*	*1,761*
Key Program Indicators		
Poverty and Food Security		
Poverty rate, individuals (%)	90.3	92.8
Eat only one meal per day (%)	21.3	19.1
Economic Activity & Productive Assets (households)		
Operate an enterprise (%)	24.1	22.6
Cultivate land (%)	95.6	96.0
Selling any crops (%)	21.6	23.9
Adult Health (age 50+)		
Morbidity (%)	55.9	49.9
Any disability (%)	13.9	14.9
Adolescent Schooling & Labor		
Ganyu work for pay (age 10-17)	41.1	38.9
School enrollment (14–17)	64.3	71.3
Safe Transitions to Adulthood		
Ever had sex (age 13-19)	34.8	31.6
Depressive symptoms (age 13-19)	44.9	50.6
Young Child Health & Nutrition		
Underweight (age 0-5)	19.3	16.9
Consumed Vit A rich foods previous day (6-59 months)	67.4	60.9
Demographic and Household Characteristics		
Per Capita Consumption (mean annual MWK)	43,891	41,357
Age (mean)	58.7	56.8
Female (%)	83.2	84.8
Chronic illness (%)	47.2	40.5
Ever attended school (%)	27.8	30.4
Married (%)	29.8	29.2
Household size (mean)	4.5	4.5
Number of shocks in past 12 months (mean)	2.5	2.5
Death in past 12 months (%)	3.8	3.2
Believes will have future financial or food shock in next 12 months (%)	53.5	53.3

Notes: No significant differences between baseline T and C groups at (p-value<0.1)

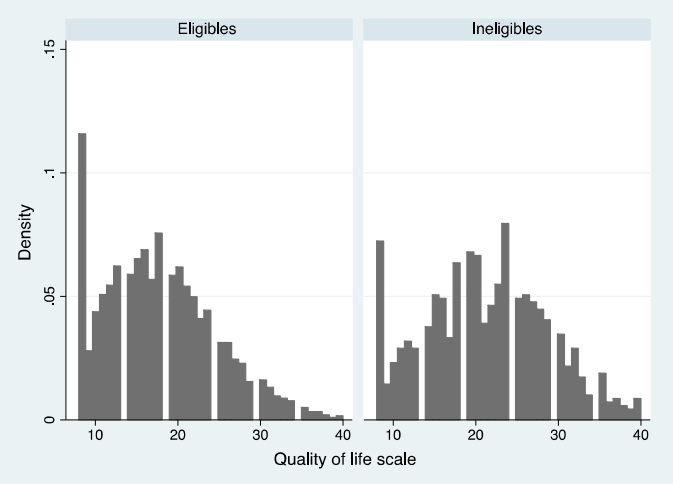
The baseline summary statistics presented in at the bottom panel of Table 2.2 show that the vast majority of caregiver respondents are female (over 80 percent) with an average age just below 60. Approximately a third of the sample has attended school at some point in their life and another third is

currently married.

The study's main evaluation focuses on six key areas and primary outcomes in these areas were tested for statistical differences between the two groups at baseline using OLS regression and accounting for the survey design to adjust standard errors. The top panel of Table 2.2 shows that randomization was successful for the panel sample used here in all key program areas. Mean characteristics between the treatment and comparison groups are balanced across these domains and there are no significant differences (p-value<0.1). In addition, all outcome and control variables used in this paper are balanced at baseline as shown in the bottom panel of Table 2.2.

At baseline, we also interviewed 821 ineligible households that were randomly selected from the same village clusters. We use this ineligible group to place our sample's responses in context and give evidence that subjective measures correspond to expected objective characteristics. Given that eligible households are the poorest households in the community, ineligible households have a number of different characteristics. Ineligible households are wealthier, have fewer household members, and household heads are younger and more likely to be married.

Figure 2.1. Histogram of QoL scale scores at baseline for eligible and ineligible households



Quality of Life:

The internal consistency of the Quality of Life (QoL) scale is respectably high with a Cronbach Alpha score of 0.83 (using both waves). Factor analysis reveals a single construct consistent with the

literature on life satisfaction scales (Frey & Stutzer, 2002). Figure 2.1 graphically shows the distribution of scores at baseline for the full eligible sample and ineligibles.

At baseline, the mean value for eligible respondents is 18 compared to 21 for Ineligibles. About 10 percent of eligible respondents report the lowest value (8) on the QoL scale, while only 0.1 percent report the highest value (40) and 95 percent of respondents have a value lower than 30. In comparison, scores for ineligibles have a more normal distribution and only 6 percent report the lowest score while 12 percent have a score over 30.

Table 2.3. Baseline values of subjective well-being for T, C and Ineligibles

	Treatment Mean (SD)	**Control** Mean (SD)	**Ineligible** Mean (SD)
QOL scale score[1]	17.5 (6.6)	18.2 (6.9)	21.2 (7.5)
Scale items			
In most ways my life is close to ideal	2.0 (1.1)	2.0 (1.2)	2.4 (1.2)
The conditions in my life are excellent	2.1 (1.2)	2.2 (1.3)	2.5 (1.3)
I am satisfied with my life	2.4 (1.3)	2.5 (1.4)	2.9 (1.3)
So far I have gotten the important things I want in life	1.8 (1.1)	1.8 (1.0)	2.1 (1.2)
If I could live my life over, I would change almost nothing	2.3 (1.3)	2.3 (1.4)	2.5 (1.3)
I feel positive about my future	2.2 (1.2)	2.3 (1.2)	2.8 (1.3)
I generally feel happy	2.3 (1.2)	2.4 (1.2)	2.8 (1.3)
I am satisfied with my health	2.5 (1.3)	2.6 (1.4)	3.2 (1.4)
Future well-being			
Better in a year	0.53	0.53	0.67
Better in 2 years	0.45	0.47	0.61
Better in 3 years	0.42	0.46	0.59
Relative well-being			
Same or better off than neighbors	0.48	0.52	0.64
Same or better off than friends	0.43	0.49	0.63
Self	1.2 (0.5)	1.2 (0.5)	1.6 (0.7)
Neighbors	1.9 (0.8)	1.9 (0.9)	1.9 (0.9)
Friends	1.9 (1.0)	1.9 (1.0)	2.0 (1.0)
Observations	*1,678*	*1,853*	*821*

[1] Range of 8-40 from the sum of scale item questions (scored 1-5)

Table 2.3 reports subjective baseline statistics for eligible T and C groups separately and

ineligibles. The mean value is around 18 for both T and C groups. Additionally, both T and C eligible respondents report middle values for life and health satisfaction (around a 2.5 out of 5), but report slightly lower values for all other items. Placing these values in context, we find higher values for ineligibles respondents living in the same communities. Their overall QoL scale score (21), both life and health satisfaction scores (2.9 and 3.2 respectively), and all other scale items are larger for ineligibles possibly reflecting the role economic hardship plays in subjective well-being for our sample. For QoL scores and all other measures in Table 2.3, we tested for balance by treatment status and find no significant differences at the 10 % level.

Future well-being:

Baseline statistics for future well-being measures in Table 2.3 show that majority of eligible households (53%) think that their life will be better in one year. However, the proportion believing life will be better decreases slightly when respondents think about their life in 2 years and 3 years in the future. Ineligible results are considerably higher but follow the same pattern of decreasing in the more distant future.

Relative well-being*:*

For relative well-being, Table 2.3 shows both respondents' placements on the economic wealth stepladder and indicators generated from these responses on whether they believe they are the same or better off than their friends and neighbors. At baseline, both T and C respondents consider themselves to be at the bottom. On a 1 (poor) to 6 (rich) scale, respondents have mean score of 1.2 or 'poor'. In comparison, respondents placed their friends and neighbors slightly higher on the scale with averages at 1.9, almost a step above themselves. Ineligible respondents perceived themselves to be slightly higher up the ladder (1.6), but had the same perception of their friends and neighbors, which might shed some light on how much relative wealth might be a shared community concept.

Empirical Analysis

This paper focuses analysis on three subjective well-being measures that represent each area discussed above. The Quality of Life (QoL) scale is a continuous measure used to measure life satisfaction while binary indicators are used for future well-being (life will be better in 2 years) and relative well-being (relative wealth is the same or above neighbors). We limit our analysis to these three measures as they are representative of overall data patterns as we found similar results using the other indicators.

Eighty-five percent of households have the same respondents at baseline and follow-up, but some households have different main respondents in the two waves. We use both the household and individual panels albeit for some different purposes. We use the household panel to measure the impact of determinants on subjective well-being and to test for sensitivity of our specifications. We also report results of the income shock using the household panel, but the individual panel is used to control for personality and individual reporting differences that could affect subjective well-being responses. Using both samples we will be able to show whether there is any apparent bias within the results using household panel when we do control for these individual, personality differences.

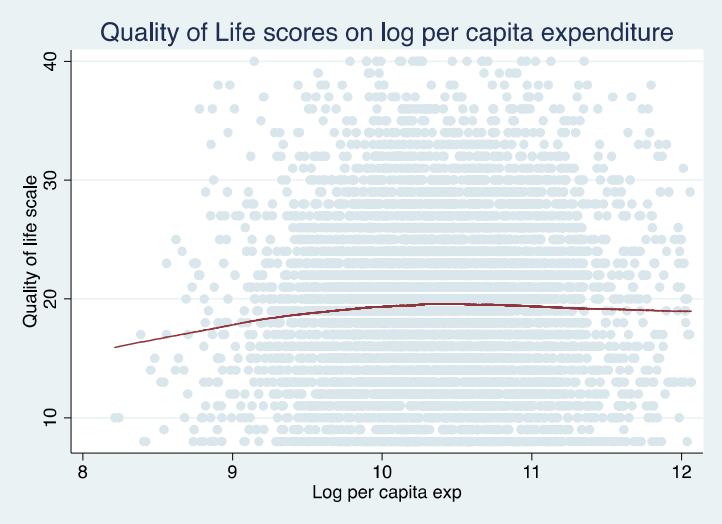
Determinants:

In the first step of analysis, we look at the impact of individual and household determinants on our subjective well-being outcomes. According the literature on correlates of subjective well-being, demographic variables such as income, sex, and age are related to individual subjective well-being but effects are relatively small. In general, positive correlates to subjective well-being include income, self-perceived good health, and marital status (being married) while female gender and age are more likely to be negative correlates (Diener et al., 2009).

Since the expectation is that a positive income shock can increase happiness, it is important to understand how consumption impacts subjective well-being. Figure 2.2 graphically represents the relationship between consumption (in logarithms) and QoL scores (range of 8-40) using a local linear

regression (Lowess) model. There is a slight rise in scores as per capita (pc) expenditure increases at lower levels of consumption, but relationship flattens at higher levels of consumption.

Figure 2.2. Lowess graph of QoL scores on household consumption at Wave 1



Although the Lowess graph in Figure 1 does not display a strong relationship, Table 2.4 shows that log pc consumption expenditure is a strongly significant determinant of QoL at baseline. In addition to consumption, there are many other determinants of subjective well-being so each of the three outcomes were tested on the full baseline sample using linear regression and controlling for individual and household covariates. According to qualitative evidence from baseline, poor health is also a significant cause of stress and anxiety for caregivers and therefore could be an important contributing factor to low subjective well-being. Table 2.4 shows that chronic illness (proxy for poor health) is an important determinant of QoL, lowering scores by 1.6 points for suffers of chronic illnesses. Additionally, being married is a strong, positive determinant of QoL increasing scores by 1.2 points over non-married caregivers. Other strong determinants of QoL at baseline include a caregiver's age (negatively associated) and household composition variables (both positive and negative).

Table 2.4. Baseline determinants of subjective well-being among caregivers (OLS)

	Life will be better in 2 years	Quality of life scale	Relative well-being: same or better off than neighbors
Treatment	-0.03	-0.87	-0.06
	(0.06)	(0.92)	(0.06)
Female	-0.04	-0.15	-0.05
	(0.03)	(0.45)	(0.02)*
Age	-0.00	-0.09	0.00
	(0.00)*	(0.03)**	(0.00)
Age squared	0.00	0.00	-0.00
	(0.00)	(0.00)*	(0.00)
Ever attended school	0.05	-0.18	0.03
	(0.03)*	(0.31)	(0.02)
Chronic illness	-0.05	-1.59	-0.02
	(0.03)*	(0.59)**	(0.03)
Married	0.05	1.16	0.06
	(0.02)**	(0.31)***	(0.02)**
Log pc expenditure	0.06	1.22	0.03
	(0.03)**	(0.40)***	(0.02)
Household size	0.01	0.33	0.01
	(0.01)	(0.18)*	(0.02)
Household Members:			
0-5 years	-0.01	-0.29	-0.03
	(0.02)	(0.24)	(0.02)
6-11 years	-0.00	-0.43	-0.01
	(0.02)	(0.19)**	(0.02)
12-17 years	0.02	-0.08	-0.01
	(0.02)	(0.17)	(0.02)
65 and over	-0.04	-0.46	-0.01
	(0.02)**	(0.21)**	(0.02)
Constant	0.12	8.52	0.16
	(0.31)	(4.47)*	(0.24)
R^2	0.06	0.06	0.02
N	3,369	3,369	3,369

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** p<0.05 ***p<0.01.

The other subjective well-being outcomes, future and relative well-being, show some similar relationships with individual and household determinants but have fewer significant ones. Log per capita expenditure is a strong, positive predictor of future well-being (p-value<0.05) but it is not predictive of relative well-being. Chronic illness is also a significant, negative predictor of future well-being. Notably, gender is only predictive of relative well-being. The heavy saturation of female caregivers in the sample,

however, means there is not much gender variation to test. The only significant determinant across all outcomes is being married, which has a positive association with subjective well-being.

In addition to the baseline sample, we use the panel of control group respondents to measure determinants—these are households that never receive the cash transfer during the study period. Table 2.5 shows the impacts of determinants using a fixed effects model to control for any unobserved differences fixed overtime. We use the household panel to pull out some impacts for variables that could change between members of households but that are fixed within individuals like gender and whether they ever attended school.

Table 2.5. Determinants of subjective well-being among control group using household fixed effects

	Life will be better in 2 years	Quality of life scale	Relative wealth: same or better off than neighbors
Time	0.05	1.62	-0.05
	(0.03)	(0.59)**	(0.05)
Female	0.07	0.84	-0.10
	(0.07)	(0.82)	(0.06)
Age	0.00	-0.16	0.01
	(0.01)	(0.08)*	(0.01)
Age squared	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)
Ever attended school	0.12	2.32	-0.09
	(0.06)*	(1.12)*	(0.08)
Chronic illness	-0.01	-1.06	0.03
	(0.04)	(0.45)**	(0.05)
Married	0.10	2.52	0.06
	(0.06)	(0.70)***	(0.06)
Log pc expenditure	0.08	0.56	0.04
	(0.04)*	(0.70)	(0.04)
Constant	-0.38	15.78	0.09
	(0.50)	(7.82)*	(0.38)
R^2	0.03	0.07	0.01
N	3,197	3,444	3,438

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** p<0.05 ***p<0.01.

Compared to the baseline sample, the impact of log pc expenditure is much smaller and less significant for future well-being (p-value<0.1) and no longer predictive of QoL. There are still a number of other significant predictors of QoL though, including positive ones such as ever attending school and being married. In contrast to the baseline sample, school attendance has a positive effect (p-value<0.1) on

QoL and future well-being for this panel of control households. Gender, however, is no longer a significant a determinant of relative well-being.

Taken together, determinant analysis for these two groups reveals that our measure of life satisfaction, QoL scale, is more strongly predicted by individual and household variables than future outlooks or relative well-being. Consumption and poor health have stronger relationships to subjective well-being at baseline than amongst the control group panel. Additionally, being married is the only determinant of all of the subjective well-being variables at baseline. In general, determinants appear to have the expected relationships with subjective well-being measures according to the literature.

Effect of treatment on subjective well-being:

Next we estimate the impact of the treatment (cash transfer) on subjective well-being. We first show the effect of log per capita expenditure on QoL scores again but with Wave 2 data to determine if the relationship between expenditure and happiness differs discernibly after receiving transfers. As mentioned earlier, per capita consumption expenditure is much lower at follow-up due to data collection occurring during the lean season. Therefore, the control group is key to our estimation strategy as it accounts for this seasonality.

Figure 2.3. Lowess graph of QoL scores on household consumption for T and C at Wave 2

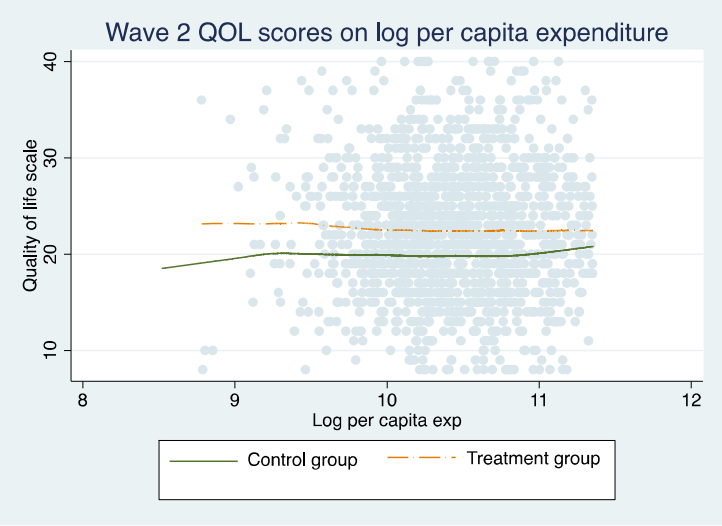


Figure 2.3 shows the relationships between QoL and consumption in Wave 2 separately for T and C groups. At Wave 2, the relationship between consumption and QoL scores has not changed much since baseline. While both treatment and control lines show little relationship between QoL scores and consumption, the treatment group clearly has higher QoL scores across all levels of consumption. This divide is largest at lower levels of consumption and weakens at the highest levels of consumption.

As described in the Methods sections, we estimate three specifications to test the impact of treatment on our subjective well-being outcomes. Results are shown in Tables 6-8. For each outcome, we start with an unadjusted model using the full household panel and then sequentially add individual and household controls. The last model includes all controls and keeps only households in the individual panel (same respondent in both rounds).

The results of treatment on subjective well-being using the OLS specification are shown in Table 2.6. The cash transfer has a positive and significant impact on caregivers' subjective well-being for QoL and future well-being. The largest effect sizes for each outcome are seen in the last models, which includes all controls and the individual panel. Caregivers in treatment households score 2.57 points greater on the QoL scale, which represents 18 percent of the mean and has a magnitude of 0.35 of a standard deviation (SD). This is similar to the Give Directly study results showing an increase of 0.45 SD in the overall index of psychological well-being for the treatment group, which is higher than the effects on happiness (a 0.19 SD) and life satisfaction (0.14 SD) individually. Additionally, caregivers receiving the cash are 18 percentage points more likely to believe in a better future. The impact of the cash transfer is strongly robust across all measures; point estimates are only slightly larger for QoL with additional controls. Furthermore, the treatment impact for QoL and future well-being maintains strong significance (p-value<0.01) across all models.

The second specification we test in Table 2.7 is a DD model with panel data to control for baseline scores and time trends. Results in Table 2.7 show that using panel data to control for baseline scores is important and makes a difference for both the magnitude and significance of treatment outcomes. In

comparison to the OLS results, QoL estimates are larger and more significant across all models (p-value<0.01). The magnitude of point estimates has increased by almost one point from 2.57 to 3.42 (or 0.5 SD) for the individual panel. Also, treatment effects on future well-being are slightly larger and strongly significant at the 1% level. The last model using the individual panel shows that caregivers in treatment are 22 percentage points more likely to believe in a better future. While relative well-being point estimates are also larger, treatment respondents are twice as likely (a 6 percentage point increase) to believe they are the same or better off than their neighbors22, results are insignificant.

Table 2.6. OLS analysis of cash transfer on measures of subjective well-being in Wave 2

	Life satisfaction (Quality of Life Scale)				Future well-being (Life will be better in 2 years)				Relative well-being (Same or better off than neighbors)			
Treatment	2.26	2.41	2.40	2.57	0.17	0.18	0.17	0.18	0.05	0.05	0.05	0.06
	(0.61)***	(0.56)***	(0.56)***	(0.53)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.04)	(0.04)	(0.04)	(0.04)
Demographics		X	X	X		X	X	X		X	X	X
Household Characteristics			X	X			X	X			X	X
Individual panel				X				X				X
Constant	19.80	20.36	15.61	13.57	0.52	0.55	0.28	0.34	0.44	0.41	0.32	0.33
	(0.25)***	(1.35)***	(2.95)***	(3.19)***	(0.02)***	(0.10)***	(0.23)	(0.25)	(0.03)***	(0.10)***	(0.26)	(0.28)
R^2	0.03	0.07	0.08	0.07	0.03	0.10	0.11	0.11	0.00	0.01	0.02	0.02
N	3,365	3,364	3,364	2,919	2,839	2,838	2,838	2,455	3,353	3,352	3,352	2,907

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01. Controls include Demographics (Female, age, age squared, ever attended school, chronic illness, married;) Household characteristics (Baseline values of log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Table 2.7. Difference-in-differences (DD) analysis of cash transfer on measures of subjective well-being

	Life satisfaction (Quality of Life Scale)				Future well-being (Life will be better in 2 years)				Relative well-being (Same or better off than neighbors)			
Treatment*Time	3.31	3.28	3.18	3.42	0.20	0.20	0.20	0.22	0.11	0.11	0.12	0.12
	(0.88)***	(0.89)***	(0.89)***	(0.94)***	(0.07)***	(0.07)***	(0.07)***	(0.07)***	(0.07)	(0.07)	(0.07)	(0.08)
Time	1.54	1.53	1.56	1.42	0.05	0.04	0.04	0.03	-0.05	-0.05	-0.05	-0.05
	(0.59)**	(0.58)**	(0.59)**	(0.61)**	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)
Treatment	-1.05	-0.89	-0.84	-0.90	-0.04	-0.02	-0.03	-0.04	-0.06	-0.06	-0.06	-0.06
	(0.94)	(0.93)	(0.91)	(0.94)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Demographics		X	X	X		X	X	X		X	X	X
Household Characteristics			X	X			X	X			X	X
Individual panel				X				X				X
Constant	18.26	19.99	11.37	10.46	0.47	0.63	0.17	0.23	0.49	0.43	0.26	0.24
	(0.68)***	(0.83)***	(2.80)***	(2.99)***	(0.03)***	(0.08)***	(0.18)	(0.20)	(0.04)***	(0.06)***	(0.19)	(0.21)
R^2	0.07	0.11	0.11	0.10	0.03	0.09	0.10	0.09	0.00	0.01	0.01	0.01
N	6,896	6,895	6,733	5,838	6,370	6,369	6,207	5,374	6,884	6,883	6,721	5,826

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01. Controls include Demographics (Female, age, age squared, ever attended school, chronic illness, married;) Household characteristics (Baseline values of log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Table 2.8. Fixed effects analysis of cash transfer on measures of subjective well-being

	Life satisfaction (Quality of Life Scale)			Future well-being (Life will be better in 2 years)			Relative well-being (Same or better off than neighbors)		
Treatment*Time	3.20	3.23	3.45	0.19	0.19	0.21	0.12	0.12	0.13
	(0.88)***	(0.88)***	(0.92)***	(0.07)**	(0.07)**	(0.07)***	(0.07)	(0.07)	(0.08)
Time	1.56	1.53	1.48	0.05	0.04	0.03	-0.05	-0.05	-0.04
	(0.60)**	(0.60)**	(0.64)**	(0.03)	(0.03)	(0.03)	(0.06)	(0.06)	(0.06)
Demographics		X	X		X	X		X	X
Individual panel			X			X			X
Constant	17.76	21.95	32.17	0.45	0.58	0.67	0.46	0.61	0.73
	(0.21)***	(1.28)***	(5.37)***	(0.02)***	(0.15)***	(0.46)	(0.02)***	(0.15)***	(0.34)**
R^2	0.13	0.15	0.15	0.06	0.08	0.06	0.01	0.01	0.01
N	6,896	6,895	5,838	6,370	6,369	5,374	6,884	6,883	5,826

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01. Controls include Demographics (Female, age, age squared, ever attended school, chronic illness, married;) Household characteristics are defined at the baseline and drop out of fixed effects models.

Interestingly, time also has a significant effect on QoL scores, an impact of about 1.5 points. Despite this time trend, the impact of the cash transfer is still larger compared to the OLS model in Table 6. Thus, controlling for baseline differences and time trends is important; it leads to slightly larger treatment effects and increases internal validity.

In the final specification, a fixed effect model, we add to the last model by introducing an individual-level fixed effect to control for any unobserved, individual heterogeneity in responses such as personality and different reporting scales. The results in Table 2.8 show that there is no change in the treatment effect with addition of individual fixed effects suggesting that unobserved heterogeneity is not a concern for this sample. Point estimates are robust for each outcome and are still significant at the 1% level for QoL and future well-being in the individual panel. The

addition of fixed effects also slightly increases the treatment impact on QoL scores amongst the individual panel to 3.45 points (0.5 SD), the largest point estimate of all models.

Additional analysis:

The results from these three specifications give strong evidence that cash transfer receipt is leading to a greater quality of life and belief in a better future, however, the income increase does not appear to impact relative well-being. Happiness literature though suggests that relative well-being could actually work as a determinant of happiness instead of a measure of well-being on its own (Weinmann et al., 2015). Therefore, in Table 2.9 we add baseline values of relative well-being (same or better off than neighbors) as an additional control to test whether perceptions of relative standing directly impacts the two other outcomes. We only compare OLS and DD specifications because fixed effect models drop these time invariant baseline covariates. Compared to Table 2.6 and 2.7 estimates, the treatment effect in Table 2.9 is unchanged (both significance and magnitude) after controlling for relative well- being at baseline. The direct effect of relative well-being is also insignificant and thus relative well-being is not a determinant of subjective well-being of our sample.

Table 2.9. Effect of cash transfer on QoL scale and future well-being controlling for baseline SWB (individual panel)

	Life satisfaction (Quality of Life Scale)		Future well-being (Life will be better in 2 years)	
	OLS	DD	OLS	DD
Treatment*Time		3.42		0.22
		(0.94)***		(0.07)***
Treatment	2.57	-0.94	0.18	-0.04
	(0.52)***	(0.92)	(0.04)***	(0.06)
Time		1.42		0.03
		(0.61)**		(0.04)
Baseline relative well-being	-0.15	-0.53	-0.01	-0.01
	(0.20)	(0.34)	(0.02)	(0.02)
Constant	13.57	10.49	0.34	0.23
	(3.20)**	(3.00)***	(0.25)	(0.20)
R^2	0.07	0.11	0.11	0.09
N	2,919	5,838	2,455	5,374

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** p<0.05 ***p<0.01. Controls include Demographics (Female, age, age squared, ever attended school, chronic illness, married;) Household characteristics (Baseline values of log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Sensitivity Analysis:

Thus far in our analysis, the QoL scale has shown high internal validity since it is consistent and robust across specifications. Internal validity, however, is also dependent on the ability of the measure to correctly represent the concept it defines; for QoL this concept is life satisfaction. As a sensitivity analysis to test the construct validity of the QoL scale, we examine whether the scale predicts negative shocks in the expected opposite direction as treatment to confirm that it incorporates appropriate emotional affect in response to one's experiences.

Using the individual panel, we test a fixed effects specification on three measures of shocks: the number of shocks in the previous 12 months, household death in the previous 12 months, and anticipation of a future shock (either financial or food) in the next 12 months. Additionally, we include treatment as a control in a second model to further see whether the cash transfer is protective of life satisfaction above these negative shocks. Since shocks could arguably be endogenous to treatment and we depart from causal analysis to validate this construct, in Table 2.2 we do show that these shocks are balanced at baseline. At follow-up, the total number of shocks in the previous year decreased from a mean of 2.5 to fewer than 2 for both groups, but the percent of the sample that experienced a death remained constant around 3 percent. Additionally, both groups were less likely to believe in future shocks at follow-up (T, 34 % and C, 44 %), declining from 53 percent at baseline.

The results in Table 2.10 show that each of these shocks has a negative relationship with QoL, serving to defend its construct validity. Each additional shock a household experienced in the previous 12 months decreases QoL scores by almost one point, significant at the 1% level. Likewise, the shock of a household member's death significantly decreases scores by 1.4 points. Belief of future shocks decreases scores by 2.3 points, also significant at the 1% level. With the addition of the treatment variable (treatment*time), both total shocks and future negative shocks still have a significant impact on QoL scores and point estimates are on the same order of magnitude. Moreover, the treatment effects in the second models are similar in size and significance as the effects found in Tables 2.8 and 2.9, validating

the robustness of the cash transfer impact on QoL for beneficiary households. Negative shocks and the positive income shock, therefore, appear to be orthogonal to each other and life satisfaction is an experience that can respond to multiple external events at the same time.

Table 2.10. Effect of negative shocks and anticipated future shocks on QoL scale using fixed effects (individual panel)

Type of Shock	Number of shocks in last 12 months		Death in household in last 12 months		Believes will have future shocks	
	(1)	(2)	(1)	(2)	(1)	(2)
Effect of shock	-0.86	-0.86	-1.41**	-1.11	-2.33	-2.13
	(0.18)***	(0.19)***	(0.67)	(0.73)	(0.47)***	(0.41)***
Treatment*Time		3.46		3.43		3.20
		(0.82)***		(0.93)***		(0.92)***
Time	2.55	0.83	3.18	1.47	2.86	1.30
	(0.60)***	(0.58)	(0.61)***	(0.64)**	(0.58)***	(0.64)*
Age	-0.47	-0.47	-0.47	-0.47	-0.42	-0.42
	(0.18)**	(0.16)***	(0.19)**	(0.17)***	(0.18)**	(0.16)**
Age squared	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)**	(0.00)***	(0.00)**	(0.00)**	(0.00)**	(0.00)**
Chronic illness	-0.84	-0.70	-0.88	-0.75	-0.95	-0.81
	(0.45)*	(0.42)	(0.45)*	(0.43)*	(0.42)**	(0.41)*
Married	1.66	1.79	1.75	1.90	1.60	1.74
	(0.65)**	(0.63)***	(0.66)**	(0.64)***	(0.67)**	(0.65)**
Constant	34.95	34.80	32.57	32.37	32.15	32.03
	(5.95)***	(5.25)***	(6.04)***	(5.36)***	(5.80)***	(5.21)***
R^2	0.14	0.17	0.12	0.15	0.14	0.17
N	5,838	5,838	5,838	5,838	5,838	5,838

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01. Household characteristics are defined at the baseline and drop out of fixed effects models.

Discussion

This study reveals that in just about a year's time, Malawi's cash transfer can have a profound effect on the subjective well-being of caregivers in beneficiary households. We find a strong, positive impact of the income shock on individuals' life satisfaction and perception of future well-being but do not find any impact on their perception of relative well-being. This finding lines up with evidence of positive impacts on objective measures of well-being at the household and individual-level. Appendix Table A1 illustrates the variety of impacts found during the follow-up midline evaluation including strong impacts on food consumption, economic productivity, school enrollment, and morbidity (Malawi SCTP

Evaluation Team, 2015). As the program aims to reduce hunger, the cash importantly helps to increase food expenditures and the number of meals eaten compared to the control group. This shows that the program was particularly protective for households during the lean season when the survey was conducted. Additionally, households used the money to increase agricultural productivity, purchasing livestock and agricultural assets, and increasing crop production. Furthermore, another main use of the cash is to help meet the costs of sending children to school; there were strong impacts on child schooling including increased enrollments and decreased dropouts (Malawi SCTP Evaluation Team, 2015).

While these positive impacts would in most respects appear to be good news, there is a concern amongst some that such programs could result in "leapfrogging". In the context of cash transfer programs, leapfrogging refers to a situation when program beneficiaries quickly move to a higher standard of living that leaves behind other community members who were nearly as poor but did not receive the program (Ellis, 2012). In this situation, feelings of unfairness and bitterness could arise and lead to lowered social cohesion within the community. This could help explain the negative subjective well-being consequences found in the Give Directly study as transfers make up a large percentage of pre-program consumption, but given that the Malawi SCTP distributes smaller transfers and only to a small percentage of the community, leapfrogging is likely less of a concern. The relative well-being results also suggest this is true since beneficiary caregivers still do not rate themselves as better off than their neighbors or friends.

This quantitative evidence from Malawi is also substantiated by qualitative evidence from in-depth caregiver interviews collected at follow-up. Caregivers in beneficiary households describe how the cash has been crucial for them to afford to eat regular meals, make home improvements, buy livestock, and send their children to school. Many of their stresses are alleviated, making them happier. Asked about personal changes since her baseline interview, one caregiver says,

> There has been an improvement in my heath and also my heart condition. I used to be very worried and stressed in the past because I had too much responsibility yet there wasn't enough money to take care of all those responsibilities. But since we started receiving money from the cash transfer program I have been able to take care of some responsibilities that I couldn't then. As a result I worry less and am usually happy which also has contributed to the improvements in my health and heart condition.

This statement highlights the importance of income to improve livelihoods for the very poor populations this program targets as well as the connection between health and happiness. Additionally, caregivers admit that they are hopeful for the future. General feelings are that they believe their lives will continue to get better and their children's future will be more promising as they are able to continue with their education. For example, another caregiver explains that she is happier and less worried now about the future because of the transfer,

> As I have said am a happy person now, I no longer have stress and am not worried because I know that when the time comes to receive the money, I will be able to buy things the household lacks now.

Moreover, she is also grateful to the government throughout the interview suggesting that gratitude and future outlooks may go hand in hand,

> Am just thankful because my household was very poor, in a rain season like this, sleep could not come because the house was leaking. We were really very poor, today my children have sleeping mats, are able to wash and bath using soap, and there is food in the household, so I say, thank you.

In addition to the substantiation from the objective measures and qualitative evidence, we find that the results of the cash transfer on subjective well-being are very robust. Both QoL and future well-being are strongly significant across all specifications and models. The cross-section OLS specification is predictably the least precise because it does not control for the time trend or baseline scores. In the other two specifications that use panel data, effects of the cash transfer are larger and change little with the addition of the controls. Even introducing treatment into regressions of negative shocks on subjective well-being does not reduce the strong, positive impact of the cash transfer. Moreover, results from Table 2.10 show that negative and positive shocks together can have strong, independent impacts on life satisfaction, possibly reflecting how positive and negative psychological states can exist simultaneously (Diener and Emmons, 1984; Watson, 1988). Literature has even found that in times of severe stress such as the death of a family member, co-occurance of aversive psychological states is common and part of the coping process (Folkman, 1997).

The positive time trend, however, is an anomaly. It is unclear why control households reported higher life satisfaction and future outlooks at the second wave. There was no concurrent rise in external economic circumstances, and in fact follow-up data collection occurred during the lean season when consumption was much lower for all households, a decline of around 25 percent from baseline (Malawi SCTP Evaluation Team, 2015). While it was the lean season, it was also the rainy season during follow-up data collection, and a possible connection could exist between the rains and subjective well-being if the rains signal that the growing season is under way and bounty is to come. However, some recent literature has rejected the use of intrapersonal comparisons (Wiemann et al., 2015). According to Rayo and Becker (2007), people develop internal references in response to life circumstances as an evolutionary response in order to sustain a minimum level of satisfaction. Therefore, individuals' criteria for a satisfied life can change overtime depending on context. It is impossible to say for sure that individuals interpret questions the same between time periods. Differences in references points at the time of survey could change the interpretation of subjective well-being questions such that an individual's 20 on the QoL scale in 2013 does not correspond the same level of happiness that a 20 does in 2015. While this could create noise in our estimates, the large sample size and experimental design help validate our results—the noise would randomly be assigned. Even withstanding this interference, we are not making conclusions about the values reported but instead are concerned about trends in the data overtime as an effect of an exogenous income shock.

Interestingly, while results are strongly positive for measures of quality of life and future well-being, we find no impacts on relative well-being. According to the literature, people's happiness is judged relative to an internal reference point, which is determined by their past experiences and environments. Therefore, the perception of low relative economic standing in a community reflects lower happiness because compared to others, there is potential to be happier. As reported in the Data section, transfer size as a share of pre-program household consumption is lower than the generally accepted 20 percent threshold for most households. It might be that this modest increase in income is not enough for

households to consume as much as their friends and neighbors and so relative to their community, they are still worse off. Therefore, the absolute income effect is likely driving the positive results we see for life satisfaction and future well-being. The null effect, however, seems to align with prior work in Malawi that found no impact of income on relative well-being among the poorest communities (Ravallion & Lokshin, 2010).

Limitations

As discussed throughout this paper, the limitations of this study mainly concern the reliability of subjective well-being measures. For one, they might suffer from measurement error because of personality bias or affect at the time of survey. Additionally, behavioral economic literature has pointed to issues that could confound results like biases of "reference points" and "habituation" (Kahneman, Diener, & Schwarz, 1999) even amongst individuals overtime (Rayo & Becker, 2007). While the time trend observed amongst control group in this study may suggest changing perceptions of subjective questions between baseline and follow-up, we do not put emphasis on the actual value of measures (the main concern expressed in the literature about reporting subjective well-being results) but instead focus on changes in trends between treatment arms. Since our results are strong and robust, and as the study uses a randomized design with a large sample size, this reduces concern that the reliability of measures is an issue.

Individual heterogeneities could also present a problem when making interpersonal comparisons of welfare as we do with the household panel. This study is strengthened by its use of experimental, panel data and methods that control of individual heterogeneities. We use fixed effects among the individual panel to wipe away personality biases and differences in interpretations. However, in accordance with recent evidence from Beegle et al. (2012) that finds biases have only a minor impact, we also find trivial differences between the household and individual panels so personality biases are not a concern in this sample.

Finally, there is concern that subjective well-being measures are not a good marker for understanding how poor people in particular are doing. People habituate and adapt to their situations and so the chronically poor may have lower thresholds for defining their well-being. The poor in India, for instance, are quick to say that they have high life satisfaction even though this does not line up with objective measures of health and productivity (Clark, 2012). Thus, their responses to subjective well-being questions could be impractical as a means of understanding how poverty affects overall welfare because their responses inadequately reflect their deprivation in areas such as health, material essentials, and education (Sen, 1990). While this would confound estimation of the relationships between poverty and subjective-wellbeing, making it harder to understand how cash transfers actually impact subjective well-being, the purpose of using subjective well-being data in this study is to compare welfare impacts of a program given to a homogenous group of poor households. We focus on data trends and do not interpret the meaning of reported values. Moreover, we do not suggest solely relying on subjective well-being to assess overall well-being and the capability of someone to rise out of poverty. We are suggesting that it could be an additional component and illustrate well-being on a more holistic level since it can incorporate other elements important to human flourishing.

Conclusion

This study shows that a positive income shock from a large-scale cash transfer program in Malawi has a strong positive impact on beneficiary caregivers subjective well-being both in terms of life satisfaction and future outlooks. The randomized, longitudinal study design combined with strong, robust impacts allows us to defend a causal relationship between income and subjective well-being. Objective and qualitative evidence from the Malawi SCTP evaluation further substantiate this evidence. Even small income increases are immensely valuable to the very poor. Caregivers use the money to improve their families' livelihoods, ensuring provision of their basics needs including food, shelter, and clothing. The reduction of these daily stresses makes caregivers happy about their current situations and gives them hope that the future will continue to get better.

The use of self-reported well-being may help capture a more inclusive picture of well-being than would reliance only on objective measures. The subjective approach is a broader concept and can include other important dimensions of a person's well-being such as social connectedness, pleasurable experiences, and life meaning (Rojas, 2015). It also is an end goal for many of the other things people seek like income—it is not desired for itself but because it can help people to achieve happiness. Nevertheless, self-reports of well-being are limited when it comes to public policy, especially given that the poor's reported happiness may minimize their true deprivation. Governments could potentially justify a lack of progress towards greater social equality by asserting that the poor are nevertheless happy. Ultimately, governments should not rely exclusively on either objective or subjective measures to judge welfare but used together they can more accurately reflect well-being.

Future research will be needed to understand if the absolute income effect will continue to have an impact on subjective well-being or if happiness will flatten out as people habituate to their new circumstances. Haushofer, Reisinger, and Shapiro (2015) find some evidence for hedonic adaptation after revisiting Kenyan households in the Give Directly cash transfer study. They find that effects for both treatment and control groups dissipated over time once the transfers stopped. However, the important distinction again between this program and Malawi's is that the Give Directly scheme provides one-time transfers compared to the more consistent, long-term transfers from the SCTP and other similar government programs.

It will also be important to investigate how greater life satisfaction can influence spending decisions and future outcomes. Cash transfer and other poverty alleviation program evaluations should continue to include subjective well-being metrics to add to this evidence base. With the growth of cash transfer programs across Africa, it will be important to find out whether there is an association between growth in these metrics and successful transition out of poverty cycle. This critical knowledge can be used to enhance the effectiveness of social protection policy for the poor across Africa.

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CHAPTER 3: CASH TRANSFERS AND HUMAN CAPITAL ACCUMULATION: CAUSAL EVIDENCE AND MECHANISMS

Introduction

The accumulation of human capital during childhood is one of the most important factors associated with adult productivity and later-life outcomes (Rosenzweig, 1995; Psacharopoulos, 1994). Education is a crucial factor in building children's human capital and it is also an important determinant in decreasing poverty and inequality (Glewwe & Kremer, 2006). Global development goals have thus focused on increasing education for all children and over the past few decades there have been vast improvements in school enrollment rates across all regions of the developing world. Most children in these regions now complete primary school and many also go on to obtain at least some secondary-level education (Glewwe & Muralidharan, 2015). Nevertheless, sub-Saharan Africa (SSA) still lags behind other developing regions of the world. Indeed, the majority of world's children that are not enrolled in school live in the SSA (United Nations, 2015). Even though many countries in the region provide free primary education, there are often other expenses like obligatory uniforms that make primary school too expensive for some families. Moreover, even if children do complete primary school, secondary education is usually much more cost prohibitive for poor households (Baird, Ferreira, Ozler, & Woolcock, 2012).

The Sustainable Development Goals (SDGs), introduced in 2015, have a more ambitious goal of universal completion of primary and secondary school. While universal access to schooling may be accomplished with heavy investments in the supply-side interventions, universal completion goals cannot be achieved without ensuring household demand for education (Bruns, Mingat, & Rakotomalala, 2003). In SSA, this household demand is unlikely to be met without reducing the barriers parents face in sending their children to school including both direct and indirect costs (Glewwe & Kassouf, 2010).

This paper analyzes the effect of a large, government-run unconditional cash transfer program on child schooling in rural Malawi. Although primarily a poverty-alleviation intervention, we examine whether and how the cash may help to increase demand for schooling. This unconditional program distributes monthly cash payments to ultra-poor households in Malawi. Transfers comprise a significant share (almost 20 percent) of pre-program per capita consumption for the average household. Households for this study were randomly assigned to either the treatment or control group after an initial baseline survey, allowing us to estimate causal program impacts. A second round of data collection occurred after approximately 12 months of payments to treatment households. If Malawi's program improves schooling outcomes, the program may help facilitate the ascent of children out of poverty given the value of educational attainment and human capital to achieve success in adulthood.

This study will also help address the gap in knowledge about how cash transfer programs impact child development. In general, children are indirectly affected by income since parents have the responsibility of making household spending decisions. Interventions that provide direct income support may increase household demand for schooling leading to greater parental investment in their children. Parental mediation is therefore a likely pathway and so we examine mediation of the program through parental channels to understand how an unconditional cash transfer affects schooling outcomes.

Schooling Interventions and Cash Transfers

Existing evidence on schooling interventions in the developing world is concentrated on how families and their children respond to supply-side interventions. Less is known about demand-side interventions such as those that attempt to directly affect investment in child human capital by relieving financial constraints. Using the traditional model of parental investment in children's human capital, a household's decision to invest in an additional year of schooling for their child occurs when the expected benefits exceed the costs with respect to the present discounted value (Becker, 1962; Ben-Porath, 1967). Policies that attempt to increase schooling attainment through enrollment or attendance therefore target

this household decision either by increasing the immediate benefits or reducing the costs of sending the child to school.

While demand-side interventions have not been as common as supply-side ones, a few types of interventions exist including scholarship programs or the elimination of school fees, and indirect programs like increasing maternal literacy or subsidizing transportation (Glewwe & Muralidharan, 2015). These interventions that either increase the immediate returns or reduce household costs to schooling have been generally effective at increasing enrollments and learning outcomes but are disparately cost-effective. Cash transfer programs, which provide cash to poor families in an attempt to alleviate poverty, have become a widespread demand-side intervention. Even though programs are poverty targeted, they also frequently aim to increase child schooling and human capital with evidence showing them to be a cost-effective intervention (Glewwe & Muralidharan, 2015). In Latin America, conditional cash transfers (CCT) like Mexico's Oportunidades program and Brazil's Bolsa Familia program do this by conditioning cash receipt on households enrolling children in school. In SSA, unconditional programs often have goals to increase school enrollment, however, they distribute cash payments regardless of recipient behavior.

While there is ample evidence that both conditional and unconditional cash transfers help children and youth stay in school (Baird et al., 2012; Robertson et al. 2013; Kenya CT-OVC Evaluation Team, 2012), given the differences between these programs, it is unclear if they work in the same ways. While conditional programs have two available mechanisms to affect household demand for schooling—the cash has an 'income effect' that helps alleviate credit constraints and the conditionality produces a 'substitution effect' that lowers the opportunity cost of schooling—unconditional cash transfer programs only work through the income effect. According to a recent review, both conditional and unconditional cash transfer programs have increased education enrollment rates in large part because they remove the financial constraints of schooling (Baird et al., 2012). Thus, if Malawi's unconditional cash transfer program reduces household financial constraints, we are likely to see households choosing to invest in an additional year of their child's education.

Mechanisms

Since unconditional cash transfer programs provide an income supplement and let households decide how to spend the money, they should only indirectly affect child well-being. The impact on child schooling thus depends upon the household response to the income, which makes it important to understand the internal allocation of resources within households (Barrientos & DeJong, 2006). The assumption is that the income affects children initially through increased household consumption resulting in a greater standard of living for the whole household, but in time, households may also reallocate resources leading to increased child investment. A few studies have examined how cash indirectly works through parental decision-making to impact child outcomes. In one such experimental study of Ecuador's conditional cash program, Atención a Crisis, authors find improvements in young children's cognitive development are associated with increased parental investment behaviors that extend beyond the direct cash effect (Macours, Schady, & Vakis, 2012).

The literature on child development has proposed a number of channels of parental behavior through which income may work to influence child schooling and human capital accumulation. The most traditional pathway, parental investment, highlights the economic component of income and argues that family income affects child development through its impact on parental decisions to allocate resources such as money and time (Becker & Tomes, 1994). Poverty limits parent's ability to provide these resources meaning poor children have less exposure to materials and experiences that could benefit their development (Mayer, 1997; Haveman & Wolfe, 1995). Using this model, income from the cash transfer would primarily have an economic effect that enables children to stay in school and build their human capital.

Another pathway, the family stress model, focuses on the role of low income and other economic hardships to inhibit child development through their effect on parental stress and emotional instability (Conger & Elder, 1994). Evidence shows that these states can lead to destructive consequences for children because they are associated with weakened relationships and harsher parenting behaviors (Guo &

Harris, 20002; Yeung et al., 2002, Gershoff et al., 2007). Income from a cash transfer could work through this pathway by decreasing financial stress and improving parental psychological well-being, which in turn improves familial relationships and parental support of their children's education.

Additionally, there may be other indirect mechanisms that could help explain cash transfer impacts on child schooling. Communities in rural SSA tend to be small and well connected such that other households are often aware of the beneficiaries. Consequently, shaming could be a factor involved in the cash transfer effect on schooling if community members observe household behaviors and think that households are not using the money appropriately. In a similar manner, households may initially believe (or be pressured from the community into believing) that there are actually rules attached to cash transfer receipt like enrolling their children in school.

Moreover, we may observe differential impacts on schooling due to a range of contextual factors such as characteristics of children and families or components of the program. For instance, evidence has shown that programs can have different schooling effects by gender such as in South Africa where unconditional cash payments had the largest impact on enrollment for girls and where female-headed households were associated with higher enrollment rates (Duflo, 2003). It is also common to see different impacts by age. Older children are less likely to be enrolled in part because free schooling typically only applies to primary school. Additionally though, time spent in school (or on school work at home) decreases the availability of children to work and so older children have a higher opportunity cost because they are more productive workers either in or out of the household. Moreover, for women, this opportunity cost includes marriage and child rearing (Glewwe & Kassouf, 2010). Another important factor is the extent of household poverty and whether the cash supplement is large enough to allow families to meet the larger costs of sending children to school. The size of the cash transfer relative to baseline consumption is therefore important since the greater the effect of the cash to reduce a household's immediate consumption needs means it can free up resources for investment purposes. Evidence has shown that the programs that distribute transfers with the largest share of household pre-

program expenditures also have the largest schooling impacts (Fiszbein et al., 2009; Maluccio & Flores, 2005). Lastly, studies have shown larger effects in populations that have lower baseline enrollments (Fiszbein et al., 2009).

The Malawi SCT Program

The Government of Malawi's (GoM's) Social Cash Transfer Program (SCTP) is an unconditional cash transfer program that aims to alleviate household hunger and poverty and also improve children's well-being and human capital. The program is targeted to ultra-poor, labor constrained households. Ultra-poor households have trouble meeting their most basic needs for both food and non-food essentials. Labor constrained households have a large dependency ratio, meaning that there are fewer wage earners or able-bodied workers to dependent members including the young, the elderly, and the disabled. These targeted beneficiaries are selected through a community-based approach with oversight provided by local and national government. If they meet these two targeting conditions, they are automatically enrolled in the program and accordingly take up is effectively universal.

The Malawi SCTP began in 2006 as a pilot program in Mchinji and an early evaluation confirms that beneficiaries are both extremely poor and vulnerable even compared to other poor households (Miller, Tsoka, & Reichert, 2010). Additionally, households have higher dependency ratios with few able-bodied household members, particularly prime-age adults, which highlights the impacts of HIV/AIDS on this generation (Handa et al., 2013).

The SCTP provides a monthly unconditional cash transfer to eligible households, which varies depending upon the number and school status of members in the household. The amount given for children in secondary school is double what is provided for children in primary school since there are school fees required for secondary school attendance. Table 3.1 shows transfer amounts in Malawi Kwacha (MWK) that were in use at time of follow-up data collection (first column, 'Prior to May 2015') and the new transfer levels that were increased in May 2015.

Table 3.1. Structure and level of transfers (current MWK)

	Prior to May 2015	After May 2015
1 Member	1,000	1,700
2 Members	1,500	2,200
3 Members	1,950	2,900
4+ Members	2,400	3,700
Each primary school child[1]	300	500
Each secondary school member[2]	600	1,000

[1]Provided for household residents age 21 or below in primary school. [2] Provided for household residents age 30 or below in secondary.
Source: Malawi Social Cash Transfer Program Midline Impact Evaluation Report (2015)

According to policy experts, the size of the transfer should amount to at least 20 percent of baseline consumption in order to have measurable impacts (Davis & Handa, 2015). In this study, households had only received the smaller transfers and the majority had a share below 20 percent. However, simulations show that after the increase, most households will be at or about this threshold.

Study Design and Data

We use data collected from an impact evaluation of Malawi's SCTP that includes both quantitative and qualitative components and was designed by UNC-Chapel Hill's Carolina Population Center and University of Malawi's Center for Social Research. IRB approval from was obtained from both the University of North Carolina (IRB Study No. 14-1933) and Malawi's National Commission for Science and Technology (IRB Study No. RTT/2/20).

The design consists of a cluster-randomized longitudinal study with a baseline and two follow-up surveys. The evaluation was designed around the GoM's plans to extend and expand coverage of the SCTP within Malawi over three years starting in 2013. In order to integrate the impact evaluation with these expansion plans, two districts were chosen for this study, Salima and Mangochi. After establishing the study districts, random selection was carried out at two smaller levels within these districts, Traditional Authorities (TAs) and Village Clusters (VCs). In the first stage, four TAs (two in each district) were randomly selected to participate in the evaluation study and then eligible beneficiary lists were generated for all VCs within these four TAs. In the second stage, with these beneficiary lists completed, VCs were randomly selected until arriving at a necessary sample size of 3,500 households based on power calculations for key program outcomes. In the end, 29 VCs were selected for inclusion in

this study and 3,531 households were interviewed at baseline, approximately 47 percent of all eligible households from the four TAs.

For our purposes, we use data from the baseline and the first follow-up surveys. The household survey is a comprehensive instrument covering household composition, consumption, economic activity, education, and health, among others. A qualitative component also includes in-depth individual interviews with the caregiver and one youth from 16 treatment households selected using a stratified sampling approach.

The quantitative baseline survey was administered over several months from June to September 2013 to the study sample of 3,531 households. Households were not assigned to treatment (T) and control (C) status until after the baseline survey in order to maintain objectivity during data collection. Half of the VCs in each TA were randomly assigned the treatment group (1,678 households) to start receiving the cash transfer right away. The other half (1,853 households) was assigned to the delayed-entry control group and entered the program in late 2015. This cluster randomization approach is preferable to household randomization in this study because it reduces concerns that treatment effects could become contaminated due to households living in close proximity with other study participants (Malawi SCTP Evaluation Team, 2013). The design is also more administratively and ethically feasible because the program did not have the financial resources to reach all households immediately.

The follow-up occurred at the end of 2014 and concluded in February 2015. Overall attrition was low at 5 percent. Additionally, detailed analysis finds no evidence of selective attrition. Beneficiary households had received five or six cash payments at the time of follow-up data collection. Each payment accounted for two months so results can be interpreted as one-year impacts of the program (Malawi SCTP Evaluation Team, 2015).

Sample

The main unit of analysis is the individual child. The sample includes all household children of primary and secondary school age (between 6 and 17) resulting in a sample size of 6,303 children.

Measures

Education

Schooling outcomes are defined for primary and secondary school aged children (ages 6 to 17) and include: school enrollment, temporary withdrawal, and dropout. School enrollment is defined as whether the child was enrolled in the current school year (2013-2014 at baseline and 2014-2015 at follow-up). Temporary withdrawal is an indicator for whether an enrolled child left school for two weeks or more during the current school year. Dropout is defined for children who were enrolled in the previous school year but not in the current school year. All measures are self-reported by the household.

Mechanisms

Parental stress is measured using the four-item shortened version of the Perceived Stress Scale (Cohen, Kamarack, & Mermelstein, 1983; Cohen & Williamson, 1988). The Perceived Stress Scale (PSS) is the most widely used psychological instrument for measuring the perception of stress. The scale intends to measure the degree to which situations in one's life are considered stressful. The PSS asks respondents to rate how often over the past month they had certain feelings that tap into how uncontrollable and overloaded respondents find their lives. The child's caregiver most often answers these subjective questions, but when this individual is not the caregiver, it is assumed that they are involved in making household decisions that impact these children. Responses are given on a one to five Likert scale from whether they "never" or "always" feel that way. Items are summed to develop a scale with a range of 4-20. In the follow-up survey, the full 10-item PSS scale was included. To test the 4-item scale for robustness, we compare the scores for the control group across these two scales. The alpha score of the shortened PSS is 0.63 across both rounds and the full scale is 0.74, the correlation between the two scales is 0.78.

Investment is measured with indicators for child specific investment. The household survey included a number of items that capture household investment in children including whether the child owns certain material items (shoes, two sets of clothes, blanket), household expenditures on child

clothing, and individual expenditures on education and health. We create index measures of investment with these child specific material items and spending measures. One index sums the number of material items (shoes, two sets of clothes, blanket) giving it a range of 0-3. The other investment index is a summation of whether the child has more than one of the three material well-being items, and whether parents spent any money on child education, health, and clothing. Therefore, the range is for this index is 0-4 with higher scores representing greater child investment.

Methods

The main estimation strategy for this paper is a Difference-in-Difference model (DD). Equation 1 shows the basic empirical specification where Y_{it} is an outcome measure for child development, T_i*P_t is the DD estimate of treatment effect, and includes indicators for treatment status (T_i) and second time period (P_t), and X_{it} is the set of control variables in the adjusted model.

$$(1)\ Y_{it} = \alpha + \beta(T_i * P_t) + \lambda T_i + \delta P_t + \phi X_{it} + e_{it}$$

The first step in our analysis is to estimate the average treatment effect of the cash transfer on schooling. Logit regressions are used for binary schooling outcomes and we report the marginal effects. Standard errors are clustered at the level of randomization, the village cluster.

In the next step, we assess how the program works on proposed meditational channels from income to schooling. Our approach is as follows: we first document treatment effects on these channels and then analyze whether any observed treatment impacts on schooling can be accounted for by the program impacts on these channels. To do this we use the causal steps proposed by Baron and Kenny (1986) that are used to establish the necessary conditions for mediation. The three equations below are modified for longitudinal analysis from Baron and Kenny's causal steps (MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002). Each meditational pathway is estimated separately and includes indicators for treatment, time, and the set of X covariates as displayed in Equation 1.

$$(2)\ Y_{it} = \alpha_{(1)} + \beta(T_i * P_t) + e_{it(1)}$$

$$(3)\ M_{it} = \alpha_{(2)} + \delta(T_i * P_t) + e_{it(2)}$$

(4) $Y_{it} = \alpha_{(3)} + \beta'(T_i * P_t) + \varphi M_{it} + e_{it(3)}$

In Baron and Kenny's model, significance is found through testing each step. Three conclusions are necessary:

1) β is significant (treatment significantly affects the outcome variable in equation 2)

2) δ is significant (treatment significantly affects the mediator in equation 3)

3) β' is not significant (when controlling for the mediator in equation 4, the previous significant relationship of treatment on the dependent variables is significantly diminished)

(Baron & Kenny, 1986)

In all models, we include controls for a child's age and sex, in addition to baseline values for being an orphan and morbidity during the two weeks prior to the interview. These variables were chosen because they are known to affect schooling and can thus improve the precision of the impact estimates. We also use controls for other variables that could affect parenting behaviors and decision-making including baseline values of the household head's sex, age, and education, as well as household characteristics including household size, total members in different age groups, consumption, and a district dummy for Salima.

Results

Table 3.2 displays the mean baseline summary statistics for schooling outcomes and all model controls by treatment arm. We test for balance between treatment arms, and after accounting for the survey design, we find no significant differences in our measures at the 90 percent confidence interval level. Summary data in Table 3.2 show that at baseline our sample is equally male and female, has an average age of 11 years olds, and more than a third are orphans. Examining baseline levels of our dependent schooling outcomes, we find that over 70 percent of children 6 to 17 were enrolled in school during the 2012-2013 school year. Out of those children, 13.5 percent had withdrawn at some point during the school year across both treatment arms.

Table 3.2. Baseline sample summary statistics by treatment arm

	Treatment Mean (SD)	Control Mean (SD)
Male (%)	51.8	51.2
Age (mean)	10.8 (3.1)	10.6 (3.2)
Past 2 weeks, suffered from illness or injury (%)	16.8	18.0
Orphan (%)	40.8	37.2
Schooling Outcomes		
Enrolled (%)	70.1	72.2
Withdrawal (%)	13.5	13.5
Dropout (%)	6.9	6.4
Literate (Chicewa) (%)	31.4	31.4
Household Characteristics		
Head went to school (%)	0.37	0.36
Head can read (%)	0.22	0.23
Head female (%)	0.86	0.86
Head age (mean)	53.1 (18.5)	51.2 (17.8)
Head widow (%)	0.38	0.35
Salima (%)	0.52	0.56
Total members 6 to 11 (%)	1.8 (1.1)	1.9 (1.1)
Total members 12 to 17 (%)	1.5 (1.1)	1.4 (1)
Total members 18 to 64 (%)	1.4 (1)	1.4 (1)
Total members 65+ (%)	0.5 (0.6)	0.4 (0.6)
Household size (%)	5.9 (2)	5.9 (2)
Baseline log per capita expenditure (mean MWK)	10.4 (0.6)	10.4 (0.6)
N	*3032*	*3292*

We also see that for both groups, dropout (left school since the previous school year) is slightly under 7 percent and about one-third of children are literate in Chicewa. Additionally, Table 3.2 shows that the vast majority of household heads are females (86 percent), only a third ever attended school, and less than a quarter can read.

Empirical Analysis

Using baseline data, we first run cross-section regressions to examine the individual and household demographic determinants of schooling outcomes for our sample. Results displayed in Table 3.3 show that age and age squared are both strongly significant individual predicators of all schooling outcomes, and particularly for enrollment. The coefficient on age for enrollment is large and positive but

negative for age squared, which signifies that enrollments are large for younger ages and start to decline as children get older. Other individual determinants include orphan status for dropouts, and male gender

Table 3.3. Baseline determinants of schooling

	Enrolled	Dropout	Withdrawal
Treatment	-0.02	0.01	0.00
	(0.02)	(0.01)	(0.02)
Age	0.23	-0.02	0.03
	(0.01)***	(0.01)*	(0.01)***
Age squared	-0.01	0.00	-0.00
	(0.00)***	(0.00)**	(0.00)**
Male	0.00	0.00	0.03
	(0.01)	(0.01)	(0.01)**
Past 2 weeks, suffered from illness or injury	0.02	0.01	0.05
	(0.02)	(0.01)	(0.01)***
Orphan	0.00	0.02	-0.02
	(0.02)	(0.01)*	(0.01)
Head went to school	0.05	0.02	0.05
	(0.02)***	(0.01)	(0.02)**
Head can read	0.08	-0.03	-0.07
	(0.03)***	(0.02)	(0.02)***
Head female	0.07	-0.01	-0.04
	(0.02)***	(0.02)	(0.02)*
Head age	0.00	-0.00	-0.00
	(0.00)**	(0.00)	(0.00)***
Head widow	-0.01	-0.01	0.03
	(0.01)	(0.01)	(0.02)
Salima District	0.06	0.01	0.00
	(0.02)***	(0.01)	(0.03)
Total members 6 to 11	0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)
Total members 12 to 17	0.01	-0.01	-0.03
	(0.01)	(0.01)	(0.01)**
Total members 18 to 64	0.01	-0.02	-0.02
	(0.01)	(0.01)**	(0.01)*
Total members 65+	0.01	-0.01	0.02
	(0.02)	(0.01)	(0.02)
Household size	-0.01	0.01	0.01
	(0.01)	(0.01)*	(0.01)*
Baseline log per capita expenditure	0.08	-0.04	-0.02
	(0.01)***	(0.01)***	(0.01)
N	6,303	4,070	4,543

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

and morbidity for withdrawals. In particular, morbidity (suffering from illness or injury in the past 2 weeks) increases withdrawals by 5 percentage-points and is significant at the 1 percent level suggesting

that withdrawals are associated with illness. Some characteristics of the household head are also determinants of schooling outcomes. Parental education backgrounds are an important factor in child schooling, particularly whether a household head ever attended school or can read. Children living with a household head that attended school are 5 percentage-points more likely to be enrolled (p-value<0.01), however, unaccountably, they are also more likely to withdrawal. Children living with literate heads are also more likely to be enrolled (8 pp) and less likely to withdraw from school (-7 pp). Household financial situations are also important; log per capita expenditure is a significant predictor of enrollment (8 pp) and dropout (-4 pp).

Next, we tested the impact of the cash transfer program on schooling outcomes (Equation 2) using both waves of data and the differences-in-differences (DD) specification defined in Equation 1. Table 3.4 shows the unadjusted and adjusted treatment impacts for each outcome.

Table 3.4. Unadjusted and adjusted impacts of cash transfer program on schooling

		Full sample of children ages 6-17		
		(1)	(2)	(3)
Enrolled		0.14	0.15	0.15
		(0.02)***	(0.02)***	(0.02)***
	N	*12,771*	*12,766*	*12,722*
Dropout		-0.04	-0.04	-0.04
		(0.02)**	(0.02)**	(0.02)**
	N	*9,001*	*8,998*	*8,968*
Withdrawal		-0.05	-0.05	-0.05
		(0.04)	(0.04)	(0.04)
	N	*9,922*	*9,919*	*9,885*
Individual controls			X	X
Household controls				X

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age, age squared male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan / Household baseline characteristics (head—female, age, age squared, ever attended school, chronic illness, married) (log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Results indicate treatment has a strongly significant effect on school enrollment and dropout (p-value<0.01). Additionally, effects for each outcome are robust to the addition of individual and household covariates. The cash transfer has a particularly large effect on school enrollment; children in treatment

households are 15 percentage-points more likely to be enrolled in school. The program also reduces dropouts by 4 percentage-points for children in treatment households.

In addition to examining treatment impacts for the whole sample, we looked at a few subgroups since certain individual characteristics (sex and age) can moderate schooling impacts for reasons such as household preferences, cultural norms, or the returns to schooling for these groups. We look at four groups separately: males, females, children who are primary school aged (6-13), and secondary school-aged children (14-17). Table 3.5 shows that the program has a robust and strongly significant impact (p-value<0.01) for enrollment across all subgroups. Males, females, and younger school-age children are either 14 or 15 percentage-points more likely to be enrolled, the same impact as on the full sample. Secondary school-age children, however, are slightly more likely to be enrolled (19 pp). For other schooling outcomes, dropout and withdrawal, there are some differential impacts by subgroup. The program has a greater protective effect on males and younger children. Both males and younger children are less likely to dropout (-6 and -4 pp respectively, p-value<0.05) and to withdraw (both -7 pp, p-value<0.10).

Table 3.5. Impact of cash transfer program on schooling by different groups

	Males	Females	Ages 6-13	Ages 14-17
Enrolled				
Treatment Impact	0.15	0.14	0.15	0.19
	(0.02)***	(0.02)***	(0.02)***	(0.03)***
N	*6,535*	*6,187*	*9,728*	*2,994*
Dropout				
Treatment Impact	-0.06	-0.03	-0.04	-0.05
	(0.02)**	(0.02)	(0.02)**	(0.03)*
N	*4,626*	*4,342*	*6,674*	*2,294*
Withdrawal				
Treatment Impact	-0.07	-0.03	-0.07	-0.01
	(0.04)*	(0.04)	(0.04)*	(0.04)
N	*5,078*	*4,807*	*7,757*	*2,128*

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age dummies, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan / Household baseline characteristics (head—female, age, age squared, ever attended school, chronic illness, married) (log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Furthermore, we examine treatment effects for those not enrolled at baseline in order to discern if the program is helping to bring children to school (either for the first time or returning) as opposed to

primarily keeping children in school. Baseline enrollment is accordingly a zero for everyone in this sample so we only use follow-up data and report single-difference treatment impacts. Results in Table 3.6 show that the program also has significant and large schooling impacts on this group. First, they are 21 percentage-points more likely to be enrolled over the control group at follow-up. Secondly, they are less likely to leave school, treatment having both a significant impact on dropouts (-11 pp) and withdrawals (-5 pp). Therefore, the program appears to have an important impact of not only keeping children in school but also helping them attend for the first time or return to school.

Table 3.6. Impact of cash transfer program on schooling for children not enrolled at baseline

	Enrolled	Dropout	Withdrawal
Treatment	0.21	-0.11	-0.05
	(0.02)***	(0.03)***	(0.03)*
N	1,935	929	1,255

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Mediation analysis

Earlier we proposed and defined potential mechanisms through which a cash transfer program might work to affect child-schooling outcomes. We identified two main parental channels—increased investment in adolescents and reductions in parental stress. We operationalize these channels with the PSS score for parental stress and index measures for child material items and child investment spending.

Table 3.7. Baseline summary statistics for mediation channels by treatment arm

	Treatment Mean (SD)	Control Mean (SD)
Total material well-being items (0-3)	1.3 (0.9)	1.2 (0.9)
Blanket (%)	35.3	34.1
Shoes (%)	20.1	17.2
Extra set clothes (%)	75.5	68.7
Total individual spending categories (0-3)	0.9 (0.7)	0.9 (0.7)
Any education spending (%)	64.5	64.3
Any health spending (%)	12.8	13.1
Any clothing spending (%)	15.7	14.8
Parental stress (PSS score) (4-20)	14.8 (3.2)	15.0 (3.4)
N	*3032*	*3292*

We first display baseline summary statistics for these potential mediation channels in Table 3.7 and test for balance between treatment and control groups. We find no significant differences between

treatment arms. Results in Table 3.7 show child ownership of material items vary by item such that while a small proportion of the sample own shoes, most children own an extra set of clothing. Our sample is also much more likely to have education expenditures over health or clothing expenditures. Finally, parental stress is towards the upper end of the PSS scale indicating high stress among child caregivers.

As the first step of causal mediation analysis, we document how the program affected these proposed parental channels. We use a DD specification (equation 3) to model the effect of treatment on our mediators. Results are displayed in Table 3.8 with the top panel showing the treatment impacts on the indicators that comprise our indices and the bottom panel showing the treatment impacts on these indices, which we further use to test parental mediation in the next step. The index measures include child material well-being items (blanket, shoes, two sets clothing), child investment (more than one material well-being item, any education spending, any health spending, any clothing spending), and the Perceived Stress Scale for the household caregiver. We also test a child investment index without education spending because expenditures are non-zero only for enrolled students, making it a strong predictor. Thus, we can compare these two indices to identify if other aspects of child investment also account for mediation.

Results indicate that the program has strong, significant impacts on most of these mediation channels amongst our school age sample. Treatment increases the likelihood of household expenditures on both child education (16 pp) and clothing (24 pp). Children in treatment households are also more likely to own two of the three material items: shoes (14 pp) and a blanket (16 pp). The index measures include these indicators and therefore it follows that treatment has large positive effects on all child investment indices. Children in treatment households have more material items (p-value<0.05) and child-specific investment spending is greater whether or not we include education as a category (p-value<0.01). Additionally, caregivers are less stressed, they score 1.2 points lower (-0.35 SD) on the Perceived Stress Scale, significant at the 10 percent level.

Table 3.8. Impact of cash transfer program on mediators

	Treatment Impact
Binary Measures	
Two sets clothes	0.06
	(0.05)
Shoes	0.14
	(0.05)***
Blanket	0.16
	(0.07)**
More than one item (clothes, shoes, or blanket)	0.19
	(0.07)***
Any education	0.16
	(0.03)***
Any health	0.01
	(0.02)
Any clothing	0.24
	(0.05)***
Index Measures	
Child material well-being items (0-3)	0.36
	(0.14)**
Child investment with education (0-4)	0.64
	(0.12)***
Child investment without education (0-3)	0.51
	(0.10)***
Perceived Stress Scale (4-20)	-1.19
	(0.59)*
N	12,722

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age dummies, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan / Household baseline characteristics (head—female, age, age squared, ever attended school, chronic illness, married) (log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Next, we re-estimate our main specification from Table 3.4 but this time include values of the mediators to test whether the treatment effect is explained in part or in whole by each mediation channel. In the rest of this analysis, we only examine mediation for enrollment and dropout because there was no observed treatment effect for withdraws and so the first condition of causal mediation analysis was not met (treatment effects schooling).

In Table 3.9, we add each mediation index measure separately into our outcome DD specification (equation 4). For all models we include the same controls used in the regressions in Table 3.4 but also add baseline values of the specific mediation measure being tested to control for any confounding between the mediator and treatment. This step is important for justifying causal claims of mediation because only

treatment was randomized and not the mediators. In order to make causal claims in this situation, the mediator should be regarded as "as-if" randomized between treatment arms. Therefore, all potential values of the outcome should be conditioned on the observed treatment and pretreatment covariates (Imani, Keele, & Tingely, 2010).

In Table 3.9 we find that, except for Columns 2 and 6, treatment effects are unchanged after adding in mediation index measures. The only measure with a mediating effect on the direct treatment impact of the cash transfer is the investment index that includes education spending. The addition of education spending within the index measure results in complete mediation of the direct treatment effect in Columns 2 and 6 since the treatment coefficient is not significantly different from zero. Since we find no impacts for an investment index without education spending, the treatment effects are being driven by the increase in education spending in beneficiary households. To understand this relationship more fully, we examine education spending in greater detail in the next section.

In additional analysis not shown here, we also test mediation for the subgroups shown in Tables 3.5 and 6. Results display the same pattern for each sample except on enrollment for older, secondary school age children (aged 14-17). Comparable to the full sample, only the investment index with education has a significant effect, however, the treatment impact is not fully mediated. Investment has a strong impact, but the program still increases enrollment by a significant 8 percentage points (p-value<0.05). If education expenditure is not accounting for the whole impact of the cash transfer for older children, it may be possible that these children have different cost barriers. The direct costs of schooling might not be the biggest barrier if they already have items like a uniform and supplies from previous school year. However, older children may have a higher opportunity cost of going to school compared to younger children because they are more productive workers and can earn income for the household. In that case, the cash transfer could be reducing the demand for children labor, allowing older children to return to school. We therefore examined child labor amongst older children as a mediation channel but find no significant effects.

Table 3.9. Impact of cash transfer program on enrollment and dropout including mediators

	Enrolled				Dropout			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.15	0.03	0.15	0.15	-0.04	-0.00	-0.04	-0.04
	(0.02)***	(0.02)	(0.02)***	(0.02)***	(0.02)**	(0.01)	(0.02)**	(0.02)**
Mediators								
Material well-being items	0.01				-0.01			
	(0.01)				(0.00)			
Investment with education spending		0.18						
		(0.01)***				-0.08		
Investment without education spending			0.01			(0.01)***		
			(0.01)				-0.00	
Perceived Stress scale				0.00			(0.01)	0.00
				(0.00)				(0.00)
N	12,418	12,410	12,410	12,721	8,842	8,834	8,834	8,967

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age dummies, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan / Household baseline characteristics (head—female, age, age squared, ever attended school, chronic illness, married) (log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Education expenditures

Given the importance of education spending, we further examine what aspects of this spending most affects schooling outcomes. Table 3.10 shows different measures of overall education expenditures to describe the extent to which education spending mediates the treatment impact on schooling. We use three measures: education expenditures in Malawi Kwacha (MWK), log of education expenditures, and a binary measure for 'any expenditures'. The top panel shows the program effect on each mediator. Unsurprisingly, the program strongly impacts all three measures (p-value<0.01). In Column 1, we see that treatment results in a 344 MWK increase in education spending. This translates to a 118% increase in expenditures (Column 2) or a 16 percentage-point increase in having any expenditure (Column 3). In the bottom panel, we include each mediator separately into the outcome regression models for enrollment and dropout and find that the treatment effect is completely mediated in all but one

model. Using a continuous measure of education expenditures (MWK), the treatment impact on enrollment in Column 1 is only partially mediated, the effect size is reduced to 9 percentage-points (from 15pp), but it is still significant at the 1 percent level.

Table 3.10. Impact of cash transfer program on schooling including education expenditure mediators

		Education Expenditures (MWK) (1)	Log education expenditures (2)	Any education expenditures (3)
T on M				
	Treatment	344.91	1.18	0.16
		(73.88)***	(0.20)***	(0.03)***
	N=12,690			
T on Y with M			**Enrollment**	
	Treatment	0.09	-0.02	-0.02
		(0.02)***	(0.02)	(0.02)
	Mediator	0.00	0.12	0.82
		(0.00)***	(0.09)***	(0.02)***
	N=12,135			
			Dropout	
	Treatment	-0.02	0.01	0.02
		(0.02)	(0.01)	(0.01)
	Mediator	-0.00	-0.07	-0.52
		(0.00)***	(0.00)***	(0.04)***
	N=8,829			

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age dummies, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan / Household baseline characteristics (head—female, age, age squared, ever attended school, chronic illness, married) (log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Since expenditures are only zero for both dropouts and children not enrolled, education spending does not vary for this group, which in large part explains the large impact of overall education spending. Since the household survey collected data on the specific categories of education spending, we further examine the spending on different categories to untangle the relationships between education expenditures and the treatment impact on schooling. For ease of interpretation and because certain categories have very low expenditures, we continue analysis using the binary expenditure measures.

Table 3.11 reports the baseline proportions of enrolled children that had expenditures in each category by treatment arm. The most common category is notebooks and stationary with approximately a

third of children in both arms having expenditures. School contributions and uniforms were the next most common categories, however, tuition is a rare expense for these children because the vast majority attends government primary schools without fees. No significant differences were found between treatment arms.

Table 3.11. Proportion of enrolled children with any expenditure on education categories by treatment arm at baseline

	Treatment	Control
Education expenditure categories:		
Tuition	0.03	0.02
Extra lessons	0.11	0.10
Notebooks & stationary	0.38	0.33
Uniform	0.15	0.14
Boarding fees	0.01	0.00
School contribution	0.31	0.24
Transport	0.01	0.00
PTA and other fees	0.14	0.10
N	*2,174*	*2,386*

Table 3.12. Impact of cash transfer program on education expenditures

	Treatment Impact
Tuition	0.01
	(0.01)
Extra lessons	0.02
	(0.04)
Notebooks & Stationary	0.15
	(0.06)**
Uniform	0.17
	(0.03)***
Boarding fees	0.00
	(0.00)
School contribution	0.09
	(0.08)
Transport	0.00
	(0.00)
PTA or other fees	-0.04
	(0.03)
N	*12,722*

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age dummies, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan / Household baseline characteristics (head—female, age, age squared, ever attended school, chronic illness, married) (log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Following the causal mediation steps, we next test the effect of the program on expenditure categories for our sample. Table 3.12 shows the DD treatment impacts on the likelihood of children

having education expenditures in each category. We find that the program has a strong positive impact on the likelihood of child expenditures on both notebooks (15pp, p-value<0.05) and uniforms (17 pp, p-value<0.01). We therefore continue analysis for these two spending categories and estimate equation 4 again including notebooks and uniform as mediators (and baseline values of each measure).

In Table 3.13 we find that for both notebooks and uniforms, the direct treatment effects on dropout are fully mediated while the effects on enrollment are partially mediated. From the original direct treatment effect (15 pp), spending on notebooks reduces the treatment effect on enrollment down to 6 percentage-points (p-value<0.10) and spending on uniforms reduces the effect to 9 percentage-points (p-value<0.01). Moreover, we find that the coefficients on both notebooks and uniforms are large and significant at the 1 percent level. Spending on notebooks has a particularly large protective effect on schooling—39 percentage-point increase on enrollment and a 13 percentage-point reduction in dropouts.

Table 3.13. Impact of cash transfer program on schooling including education expenditure mediators

	Enrolled		Dropout	
Treatment	0.06	0.09	-0.02	-0.03
	(0.03)*	(0.02)***	(0.02)	(0.02)
Mediators				
Notebooks & Stationary	0.39		-0.13	
	(0.03)***		(0.01)***	
Uniform		0.17		-0.05
		(0.01)***		(0.00)***
N	12,172	12,172	8,865	8,865

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age dummies, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan / Household baseline characteristics (head—female, age, age squared, ever attended school, chronic illness, married) (log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Finally, we also examined other explanations for observed schooling impacts in analysis not shown here. We tested other potential schooling moderators—factors that could affect schooling outcomes but would not be impacted by the cash transfer program and so would not plausibly mediate the impact from treatment to schooling. Neither of the factors we tested, the time it takes to get school and whether there is a school-feeding program, has a significant effect on schooling. We also examined other potential mediators—household food consumption, transfer share, and child labor—and find no mediation

of these measures either. Finally we asked treatment households whether they believed that rules existed about sending children to school to receive the cash transfer. When we examine rule perception among treatment households at follow-up, we also find no significant effects on enrollment or dropout.

Discussion

In this study we show that Malawi's unconditional cash transfer program, the SCTP, is an effective demand-side education intervention. The cash helps poor children to attend school by alleviating the financial burden of schooling for the household. Specifically, school-age children (aged 6-17) in treatment households are 15 percentage-points more likely to be enrolled in school and 4 percentage-points less likely to dropout. Furthermore, examining the impacts by individual sub-groups, we find that impacts are very similar by gender and age. Both males and females in treatment households are significantly more likely to be enrolled in school over the control group and the magnitude of treatment effects are comparable to the full sample. Treatment effects on enrollment are also significant for both primary and secondary aged children and dropouts are significantly reduced for all but females. We also find that for those children that were not enrolled at baseline, treatment effects are even stronger (21 pp for enrollment and -11 pp for dropouts) implying that the cash is helping children return to school or permitting them to go for the first time.

We also investigated mechanisms through which the cash may work to improve schooling outcomes. As the literature highlights how income affects children indirectly through parental decisions, our main analysis investigates mediators of parental investment and stress. In addition though, we examined other explanations for observed schooling impacts including channels of household food consumption, transfer share, and child labor. Our results indicate that impacts are entirely explained by parental investment in the form of increased education related spending.

Since the cost of schooling is the biggest factor for these poor families in the decision to send their children to school, the cash works mainly by alleviating some of these economic constraints. Our results show that education spending completely mediates the direct treatment impact, but further analysis

shows that in particular, the cash is spent on notebooks (or other stationary) and uniforms. Out of all education expenditure categories, these two items are the only ones that the program significantly impacted, and we find that they both individually help mediate the direct treatment effects on enrollment and dropout. It is clear from our analysis that the income effect of the cash transfer is primarily responsible for the positive schooling impacts we find, parents can now afford certain schooling items for their children, notebooks and uniforms specifically, that help children attend school.

While these specific purchases help explain why the cash transfer is improving school enrollments and dropouts, claims that these items cause the observed schooling impacts is not very intuitive. However, in the qualitative data[1] we do find that acquisition of these items is a frequently cited reason for why children can attend and stay in school. For example, although officially primary education is free and uniforms are not compulsory, sometimes schools will not allow children to attend. Such as was the case for one male youth respondent,

> What really made me drop out is the lack of money to pay for what I have just told you but also I had no school uniform, so they sent me back from school.

Additionally, youth commonly described a stigma of being without certain school items such that they could suffer ridicule by their classmates and teachers if they lacked them. For instance, respondents at baseline discussed sometimes being bullied by teachers or school administers for dirty uniforms or lack of supplies. Moreover, the most cited reason for missing or dropping out of school was not having the basic school supplies, although other reasons included competing demands on their time such as needing to do informal wage labor (ganyu) to support the household and for girls, taking care of children.

The follow-up qualitative interviews also provide support to the story emerging from the quantitative data that the cash transfer works to improve schooling outcomes because it increases education expenditures on certain items. Interviews from both caregivers and youth often mention that the reason the cash is helping them in school is because it enables the purchase of uniforms, soap, and school

[1]Qualitative data comes from baseline and follow-up in-depth individual interviews with a caregiver and one youth from 16 treatment households selected using a stratified sampling approach.

supplies. Caregivers, in particular, frequently discussed how the money is important in sending kids to school with clean uniforms and school supplies. For example one caregiver says,

> We use the money to buy washing soap so that the children should put on clean clothes when they are going to school. I also use the money to buy learning materials like notebooks and pencils, sometimes the school demands a small amount of fee in which case we also use the money from the cash transfer program.

These changes are also described as helping to facilitate the entire school experience including feeling socially accepted and academically engaged. The same caregiver says about one of her children,

> …[Child's name] was not working hard in class because we didn't have enough money to help her with her education. But she now works hard because we started receiving money from the cash transfer program.

In addition, youth also explained how the program directly led to improvements in their school experiences. For example, one male orphan explains how the cash has made a difference since baseline,

> In the past I used to miss a lot of classes because I had no clothes. But now I have enough clothes, including a school uniform. I hope that I will continue with school…I had no hope of continuing school the last time we talked because of what was happening to me.

While many youth explained how the cash is helping them or other children of the household attend school, in some cases, the cash was not enough to overcome the financial costs. One female simply states why she stopped, "Poverty is the reason, lack of clothes, and lack of soap." Another states that he would like to return and admires his friends in school, but to return what he needs is, "Money…[it] would help me to get some of the necessary things required for school [like] school uniform, notebooks and pencils." In his case, the money from the cash transfer was being used for other purposes such as food and caring for his disabled mother and so it was not enough to help him attend school.

Moreover, another issue for older children is the cost differential of attending secondary school. In one youth's case, he had completed primary school and started to attend secondary school but the cash transfer was not enough for his family to afford the increased fees and so he was sent home for not paying them. One last challenge that youth expressed in attending school was not the financial cost but other responsibilities and demands on their time. For example, although one female wanted to return to school after her households started receiving the cash, she did not have anyone to watch her young child.

Limitations

One limitation of this study is that there are fundamental challenges in isolating the ways in which income affects child development outcomes. For one, these pathways are mostly unobserved and endogenous to the household (Strauss & Thomas, 2008; Shadish et al., 2002). Moreover, the measures we have of parental stress and investment are latent constructs for the true parental behaviors and thus we may be imprecisely measuring their impact. Finally, the causal mediation literature shows the strongest identification test would require randomization to mediator levels but our measures are not externally manipulated meaning the model may lack some predictive power (Bullock & Ha, 2011). Nevertheless, the strength of our study design, including the longitudinal data and randomizing economic conditions, and the use of non-experimental methods, means that this analysis offers reasonably strong evidence for these mediation results.

Another limitation is that we are limited in testing short-term outcomes since we do not have measures of achievement or cognitive aptitude. The goals of the SCTP program are to increase child human capital and although we cannot directly test for this, evidence from Malawi shows that there are relatively large returns to schooling; the average Mincerian rate of return to years of schooling is 10 percent (Chirwa & Matita, 2009). Moreover, just gaining basic literacy and numeracy skills are valuable for participation in economic activities throughout life. Therefore, prolonged school attendance that leads to grade completion is an important factor in improving children's later-life outcomes.

One final limitation is that expenditures on educational items are only collected for enrolled students. In order to better understand the impact of educational resources on schooling, we would ideally collect ownership of material items in addition to expenditures for all school-age children. However, given that our results are strong and robust across the qualitative evidence, it appears that we are identifying the most likely material items that are producing improved schooling outcomes.

Conclusion

This study provides causal evidence from Malawi's SCTP that an unconditional social cash

transfer program can have strong effects on school attendance for children in beneficiary households, and it works by relieving some of the financial barriers of schooling. More specifically, it helps families to purchase uniforms, notebooks, and other school supplies. Although improving schooling and child human capital is an objective of the SCTP program (and many similar programs across the developing world), there is no obligation for families to send their children to school to receive the money. Therefore, our findings indicate that parents are eager to invest in their child's education, and by helping families meet the costs of schooling, unconditional cash transfers can directly increase the demand for education.

Although Malawi's SCTP may help children enroll and stay in school, it is not clear, however, that this will lead to greater human capital accumulation. Malawi's education system is stressed—there are not enough teachers or classrooms, schools are overcrowded and dilapidated, and facilities often lack proper sanitation or clean water. Even if programs are successful in increasing short-term outcomes such as enrollment, poor educational quality is a threat to achieving medium-term outcomes such as greater student achievement. Therefore, it may be that efforts to improve the demand for education through cash transfers will be undermined without improvements to poor-quality educational systems.

Intuitively, we would expect that supply-side investments are important but evidence from other countries is ambiguous as to how they interact with cash transfers to impact both short-term and medium-term outcomes. More evidence is needed on whether cash transfers that are accompanied by supply-side interventions lead to improvements in both short-term outcomes like enrollment and medium-term outcomes like improved educational achievement. However, even if quality improvements are gradual, schooling appears to be one of the most promising pathway through which cash transfers may contribute to the successful transition to adulthood. Recent cash transfer evidence has been showing the protective relationship school attendance appears to have on a number of child and adolescent development outcomes such as early pregnancy (Handa et al., 2015), sexual behaviors, (Baird, Garfein, McIntosh, Özler, 2012) and mental health (Baird, de Hoop, & Özler, 2013; Kilburn, Thirumurthy, Halpern, Pettifor, & Handa, 2015).

Overall, this study contributes to emerging evidence on the influence of social cash transfer programs in SSA to promote child development by targeting household poverty. Results reveal that within a relatively short amount of time, unconditional cash programs can improve child-schooling outcomes and that parents will invest resources in their children even without an explicit condition. Implications are that in these ultra-poor contexts where enrollments are lower than socially desired, this type of poverty-targeted cash transfer program could result in large, cost-effective improvements in child schooling and human capital. Policymakers should therefore be conscious of the potential efficacy of these programs to meet world development goals by increasing the demand for education.

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CHAPTER 4: CASH TRANSFERS AND ADOLESCENT MENTAL HEALTH: CAUSAL EVIDENCE AND MECHANISMS

Introduction

Mental health problems that develop during childhood and adolescence may have significant and long-lasting impacts because proper psychosocial functioning is an essential factor in the developmental process for young people. Adolescent mental health is particularly important for a successful transition into adulthood because of its association with other important factors like physical health, educational attainment, and risky or delinquent behaviors (O'Connell, Boat, & Warner, 2009). Moreover, good mental may be important during adolescence because is strongly associated with positive affect (Fredrickson & Losada, 2005) and evidence has shown that positive affect benefits decision-making by increasing focus, self-discipline, and the ability to make forward-looking decisions (Isen 2008; Lyubomirsky, King, & Diener, 2005).

One of the biggest influences on child and adolescent development is socio-economic status. Poverty during childhood can have detrimental effects on children's mental health in addition to their human capital and physical health. These developmental disadvantages can accumulate and contribute to the persistence of poverty as they stick with individuals into adulthood (Yaqub, 2002; Harper, Marcus, & Moore, 2003). A sizeable literature in the developed world has found that the negative impacts of poverty on child development can be improve with increases in household income (Brooks-Gunn & Duncan, 1997; Gershoff, Aber, & Raver, 2007; Mayer, 1997; McLoyd, 1998) including causal evidence from both natural and experimental designs (Costello, Compton, Keeler, & Angold, 2003; Morris & Gennetian, 2003). These studies have linked increases in income to changes in parental behaviors that support improved child development outcomes.

In the developing world these relationships have not been thoroughly examined, however, poverty-alleviation programs that directly increase household income through cash payments present an opportunity to provide evidence from these settings. These social cash transfer programs (SCTs), by reducing poverty, may help address the burden of mental health among poor adolescents, boosting their chances for long-term success. Some studies have previously linked cash transfer receipt to improved mental health outcomes (Lund et al., 2011; Baird, de Hoop, & Özler, 2013; Kilburn, Thirumurthy, Halpern, Pettifor, & Handa, 2015) and globally, there is increasing evidence to suggest a protective effect of SCTs on young people. Notably, cash transfer programs may have the potential to reduce risky sexual behaviors (Baird, Chirwa, McIntosh, & Özler 2010; Heinrich, Hoddinott, & Samson, 2015; Cluver et al., 2013) and HIV incidence in high HIV/AIDs prevalence settings (Baird, Garfein, McIntosh, & Özler, 2012). These outcomes might even be bolstered by improvements in mental health since risky sexual behaviors have been linked to mental health problems in youth (DiClemente et al. 2001; Ramrakha et al., 2000).

Yet, despite this mounting evidence, the potential for cash transfers to improve the transition to adulthood remains a question. In particular, there is little understanding of how income from the cash transfer impacts these adolescent outcomes because cash is given at the household level and thus indirectly impacts adolescents through parental spending decisions. This study will help address this gap by analyzing the impact of a cash transfer program on adolescent mental health and some of the possible intervening pathways.

In this paper, we use a randomized study of a social cash transfer program in Malawi to identify the causal effects of a positive income shock and mediating pathways on the mental health of adolescents ages 13-22. Malawi's Social Cash Transfer Program (SCTP) is an unconditional, government-run program that distributes monthly cash payments to rural, ultra-poor households. Unconditional cash transfers distribute payments regardless of recipient behaviors, allowing households to spend money how they wish, whereas conditional transfers tie receipt to certain conditions (Handa, Devereux, & Webb,

2010). Transfers from the SCTP comprise a significant share (almost 20 percent) of baseline per capita consumption for the average household in our study. The households for this study were randomly assigned to the treatment or control arm after an initial baseline survey and a second round of data collection occurred after approximately twelve months of transfers were distributed to beneficiary households. This experimental study design allows us to estimate causal effects of the program on mental health and possible mediators.

Using the short form of the Center for Epidemiological Studies Depression scale (CES-D), a commonly used instrument to assess depression prevalence in field settings, we show that a positive income shock does not result in a significant reduction in depressive symptoms for most adolescents. In the year's time frame of this study, adolescents in treatment households at follow-up had lower average CES-D scores and depression prevalence rates, but results are not significant for the full sample. However two subgroups, orphans and females, did experience declines in depression with significance at the 10 percent level. We do not find strong evidence of mediation for any of the intervening pathways that we investigate including parental stress, investment, material well-being, schooling, or illness. School enrollment, however, appears to be a significant protective factor for adolescent mental health.

Mental Health and Poverty

Adolescent mental health is a significant global health concern as disorders comprise a major share of the disease burden for young people (World Health Organization, 2004) and because disorders put adolescents at a higher risk of mortality through their link to suicides, a leading cause of death amongst this age group (Prince et al., 2007). Moreover, many of the other leading contributors to the burden of disease in young people have mental-health dimensions. For example, chronic diseases, substance abuse, and violence in young people are associated with an increased risk of mental disorders. Mental health problems, on the other hand, can also predispose exposure to abusive environments (Patel, Flisher, Hetrick, & McGorry, 2007).

Another particularly relevant issue for adolescents in Malawi and other sub-Saharan African

(SSA) settings is the link between mental health and HIV/AIDs. For one, young people with mental disorders are at a higher risk of contracting HIV/AIDS with the increased risk associated with factors such as low self-efficacy and exposure to peers and situations that encourage risky behaviors like unprotected sex (Lyon et al., 2000; Donenberg & Pao, 2005). Reciprocally, having HIV/AIDs also puts adolescents at greater risk of depression. Moreover, there are mental-health consequences of having family members that are currently affected or were lost by the disease (Cluver, Gardner, & Operario, 2007).

Proper psychosocial functioning also plays a fundamental role in an individual's quality of life given that mental disorders are associated both with internal suffering and external issues like discrimination. Moreover, good mental health for adolescents is additionally significant since mental disorders in young people tend to persist into adulthood (Costello, Foley, and Angold, 2005; McLaughlin et al., 2010) and can have long-term consequences on their future well-being (McLoyd et al. 2009; Knapp et al., 2002). Primarily, mental health problems during adolescence can impair development across cognitive, economic, and social domains. For instance, evidence links child mental health problems to worse educational and labor market outcomes (Currie & Stabile 2006; Fletcher & Wolfe 2008; Gibb, Fergusson, & Horwood, 2010; Currie & Madrian, 1999), poor sexual health (DiClemente et al., 2001), as well as substance abuse and violence (Patel et al., 2007). Furthermore, poor mental health can also lead to increased risky decision-making, which has implications that can extend into adulthood as adolescents are making decisions related to schooling, employment, and sexual activity (Patel et al., 2007).

The burden of mental health problems for young people is particularly heavy in low-income countries, a relationship that is underscored by increasing evidence that poverty and mental health are linked in a two-way relationship. Poverty can help to maintain or act as a catalyst for mental health problems through channels such as chronic stress, social exclusion, and exposure to trauma or violence. On the other hand, mental illness can increase the risk of poverty through reduced productivity, loss of employment, and increased health expenditures (Lund et al., 2011). While the evidence for this relationship in high-income countries is fairly robust (Patel et al., 2010), in the developing world, the

evidence linking poverty and mental health is inconclusive. In a recent review, Lund et al. (2011) examine interventions that either addressed mental health or economic well-being and find mixed evidence on the connection between common mental disorders and economic well-being. Interventions that targeted mental health had stronger impacts on economic and financial well-being than did economic interventions on mental health.

On the other hand, evidence on the effect of income shocks on mental health provides some support for this two-way relationship. For example, natural experimental evidence from Britain and Sweden lottery winners give some of strongest evidence of the benefit of a positive income shock on mental health (Gardner & Oswald, 2007; Lindahl, 2005). Additionally though, there is growing evidence that cash transfers can lead to decreased stress and improved mental health in the household. Give Directly, an unconditional cash transfer program in Kenya that distributes large one-time payments, found significant impacts on psychological well-being among beneficiary adults including reduced depression using the full CES-D scale (Hausofer & Shapiro, 2013). A couple of recent studies have also found improvements in adolescent mental health outcomes. Baird, de Hoop, and Özler (2013), for one, find evidence for improved psychosocial health from a pilot RCT in Malawi that randomized schoolgirls to receive a monthly cash transfer, which included both a conditional and unconditional arm. Significant mental health effects, measured with the GHQ-12, were found for both treatment arms while the program was ongoing, but the effect dissipated after the transfers stopped. Additionally, Kilburn et al. (2015) find that a national, unconditional cash transfer program in Kenya, the CT-OVC, had a significant, positive impact on young people's mental health 4 years after the program began.

Neither of these studies, however, collected baseline data on mental health. This paper therefore improves upon the current literature by including baseline levels of mental health and following up after a year. However, we note that the scope of SCTP program to impact depressive disorders after a year are mostly likely limited to those which are temporary in nature and affected by environmental factors as the opposed to full scope of disorders that have greater biological associations and are longer in duration

(Deater-Deckard et al., 1997).

Pathways of Influence

In addition to providing evidence on the relationship between poverty alleviation and mental health by asking whether a cash transfer program can decrease adolescent depression, another important question is, how might this income work to benefit adolescents? In the case of the Malawi SCTP and other similar programs in SSA that distribute unconditional cash, this is especially important since it is up to households to decide how to spend the money. In this paper, we therefore investigate a few pathways through which the SCTP could impact adolescents in order to better understand the relationship between poverty alleviation and adolescent mental health in this setting.

Conceptually, there are a number of channels that the cash transfer may operate through to influence adolescent mental health. Firstly, there is abundant evidence that cash transfers help improve household welfare by decreasing poverty and managing economic shocks including evidence from the Malawi SCTP (Miller, Tsoka, Reichert, 2008; Malawi SCTP Evaluation Team, 2015). In particular, the cash is often used to increase household food consumption, nutrition, and spending on health care (Adato & Basset, 2009). Thus, the cash may work to improve adolescent mental health through improvements in their health status since physical and psychosocial health are strongly linked to each other (Patel et al., 2007). Secondly, improved economic situations may also increase resource allocation to children such as spending on items to improve their material well-being. In Malawi, the SCTP has helped improve child material well-being through purchase of personal consumption items including shoes, clothing, and hygiene products. These items may improve self-esteem, especially given the stigma reported in qualitative interviews of going to school without shoes or clean uniforms (Malawi SCTP Evaluation Team, 2015). A third potential channel is through increased school enrollment and attendance as evidence from both unconditional and conditional programs has shown positive impacts of cash transfers on adolescent schooling (Baird, Ferreira, Özler, & Woolcock, 2013). School attendance may be protective of adolescent psychosocial well-being in part because it is associated with improved self-esteem (Heckman,

Stixrud, & Urzua, 2006) but additionally, it may decrease exposure to risky environments given the time commitment of school and higher quality, focused peers.

Although these channels may help explain the ways cash can improve adolescent mental health, parental behaviors serve as the foundation of these channels because parents play the primary role in resource allocation and household decisions related to schooling and work for adolescents. A robust literature on mediation between income and child development has proposed a number of behaviors including parental psychological well-being, investment of money or time in children, and aspects of parenting behavior such as harsh discipline or affection (Conger & Elder, 1994; Haveman & Wolfe, 1995; Mayer, 1997). Two of these pathways are examined in this study, parental ability to invest resources in their children and parental psychological well-being in terms of their stress levels.

The parental investment model focuses on the economic effect of income to enable the purchase of resources for child investment (Becker, 1991; Becker & Tomes, 1994). In this model, poverty affects parental behavior by limiting parents' ability to invest money and time in their children, lowering children's exposure to materials and experiences that would benefit their development. Empirical results have shown that this behavior is a pathway between income and child development but primarily for cognitive outcomes and has less of an effect on socio-emotional outcomes (Guo & Harris, 2000; Linver, Brooks-Gunn, & Kohen, 2002; Yeung, Linver, & Brooks-Gunn, 2002). The second pathway of mediation, the parental stress model, focuses on how economic hardships such as low income contribute to stress and emotional instability (Conger & Elder, 1994). These states are associated with harsher and less supportive parenting, which then leads to negative developmental consequences for children. Empirical work has confirmed these relationships, particularly for child social and emotional behaviors **(**Guo & Harris, 2000; Yeung et al., 2002, Gershoff et al., 2007).

Families living in extreme poverty are important in understanding these relationships because more than other households they tend to experience chronic stressors and negative shocks that can place them under severe financial strain (Adato & Bassett, 2009). Furthermore, in SSA, HIV-affected

households are particularly stressed, research showing the disease is associated with increased poverty (Gillies, Tolley, & Wolstenholme, 1996), caregiver depression (Lachman et al., 2013; Littrell et al., 2012), increased child behavior problems (Sipsma et al., 2013), and lack of social support both emotionally (Casale & Wild, 2012) and financially (Heymann & Kidman, 2009).

If the cash helps to improve household economic conditions, this will strengthen the capacity of parents to provide better care for their children and could subsequently improve parenting decisions. If parents start to spend more on items that improve adolescent well-being such as nutritious foods, education, or health care, this may positively impact mental health. Moreover, increased household income may improve mental health among adolescents as reduced financial stress among parents can decrease adverse moods and improve relationships.

This paper will examine the effect of the SCTP on these two parental pathways and whether they help mediate any impact on adolescent mental health. These parental pathways should account for several of the proposed mediation channels, however, some of these channels may have additional direct impacts not explained by parental behavior. Consequently, we also examine individual adolescent variables including health and schooling. Furthermore, individual and household characteristics may modify the impacts of the cash transfer on either depression or pathways. In particular, gender could be an important modifier since most common mental disorders have a characteristic gender divide such that young women are 1.5–3 times more likely to have depressive disorders (Patel et al., 2007). Other important characteristics include being an orphan since losing a parent puts young people at greater risk of depression (Pao et al., 2000) and depth of poverty given the greater marginal impact the income from the cash transfer could have for the neediest households.

Measures

Mental Health Instrument

Adolescent mental health is measured using the CES-D10, a 10-question short version of the 20-question Center for Epidemiological Studies Depression (CES-D) test that measures depressive symptoms

for individuals (Radloff, 1977; Andresen, Malmgren, Carter, & Patrick, 1984). The short form is not a diagnostic tool but is used to measure the current level of depressive symptoms by focusing on the affective component of depressed mood. The CES-D10 has been validated across a variety of geographical contexts and demographics (Andresen et al., 1984) and is known to have good internal consistency.

The CES-D10 contains ten items answered on a one to five Likert scale (listed in Appendix Table B1). Questions ask how often certain feelings or behaviors occurred in the past seven days, responses include "Rarely" (<1 day), "Some or a little of the time" (1-2 days), "Occasionally or a moderate amount of time" (3-4 days), "Most or all of the time" (5-7 days). Responses receive a score from one (rarely) to four (most of the time) and higher scores reflect more depressive symptoms. Scores are summed across all questions to create a scale where higher scores reflect more depressive symptoms. The scale has high internal validity across both waves with a Cronbach Alpha score of 0.73. The scale is adjusted down to a base of zero (range of 0-30) and scores of 10 or higher are classified as displaying depressive symptoms. This cutoff has been used in previous studies in SSA settings (Asante & Andoh-Arthur, 2015; Othieno et al., 2014; Kilburn et al., 2015), but this cutoff has not been validated in Malawi and we did not validate this threshold with psychiatric interviews. Therefore, we examine the robustness of our findings by examining the program impact using other cut-offs and the continuous CES-D scale scores.

Mechanisms

Parental stress is measured using the four-item shortened version of the Perceived Stress Scale (Cohen, Kamarack, & Mermelstein, 1983; Cohen & Williamson, 1988). The Perceived Stress Scale (PSS) is the most widely used psychological instrument for measuring the perception of stress. The scale intends to measure the degree to which situations in one's life are considered stressful. The PSS asks respondents to rate how often over the past month they had certain feelings that tap into how uncontrollable and overloaded respondents find their lives. Responses are given on a one to five Likert scale from whether they "never" or "always" feel that way. Items are summed to develop a scale with a range of 4-20. In the

follow-up survey, the full 10-item PSS scale was included. To test the 4-item scale for robustness, we compare the scores for the control group across these two scales. The Cronbach Alpha score of the 4-item PSS is 0.63 and 0.74 for the full scale; the correlation between the two scales is 0.78.

Parental investment is measured using child specific spending and individual ownership of certain items. Included in the household survey was an extensive expenditure module that captured household expenditures on child clothing, and individual expenditures on education and health. Additionally, for children 5 to 18, we collected data on whether the child owns certain material items—shoes, two sets of clothes, blanket. We create two index measures of investment with these variables. The first, child specific expenditures, measures whether the household spent any money on child education, health, and clothing. The second, child's material well-being, is a summation of whether a child owns shoes, two sets of clothes, and a blanket. Therefore, both indices range from 0-3 with higher scores representing greater investment in the child. We test each measure of investment (individual expenditure and material items) and each index for program impacts.

The Malawi SCTP

The Government of Malawi's (GoM's) Social Cash Transfer Program (SCTP) is an unconditional cash transfer program targeted to ultra-poor, labor constrained households. SCTP beneficiary selection is made through a community-based approach with oversight provided by local and national government. Ultra-poor households are unable to take care of members' most basic needs including food and essential non-food items such as clothing. Labor constrained households have more dependent members than wage earners or those able to work. Household members are dependent if they are below 18 or above 64 years old, or if they are between age 18 and 64 but have any illness, disability, or other condition making them unable to work (Malawi SCTP Evaluation Team, 2014).

An early evaluation of the Malawi SCTP in Mchinji confirms that recipient households live in extreme poverty and have higher dependency ratios than other poor households (Miller, Tsoka, & Reichert, 2010). Additionally, household heads tend to be older (above 60) and upwards of 80% of

households are missing at least one prime-age adult, highlighting their particular vulnerability to the impacts of HIV/AIDS (Malawi SCTP Evaluation Team, 2013).

The monthly transfer provided by the SCTP varies in size depending upon the number and school status of members in the household. Table 4.1 shows transfer amounts in Malawi Kwacha (MWK) that were in use at time of follow-up data collection. The average cash transfer amounts to around $8 per month.

Table 4.1. Structure and level of transfers

	Transfer Size (MWK)
1 Member	1,000
2 Members	1,500
3 Members	1,950
4+ Members	2,400
Each primary school child[1]	300
Each secondary school member[2]	600

Source: Malawi Social Cash Transfer Program Midline Impact Evaluation Report (2015)

The transfer size as a share of pre-program household consumption is an important factor to consider when assessing program effects with 20 percent being the proposed 'rule of thumb' in order to have measurable impacts (Davis & Handa, 2015). The average transfer share in our sample was 18 percent at follow-up, however, beginning in May 2015 transfer amounts were increased so that majority would meet or exceed this 20 percent threshold.

Research design and sampling

The impact evaluation of Malawi's SCTP study uses a cluster-randomized longitudinal design. The evaluation was designed around the GoM's plans to expand coverage of the SCTP across Malawi over three years starting in 2013. In order to integrate the impact evaluation with early expansion plans in 2013, two districts, Salima and Mangochi, were chosen for this study. Random selection was included at all possible levels within these districts including Traditional Authorities (TAs) and Village Clusters (VCs). Since the program did not have the financial resources to reach all households immediately, the study design is both administrative feasible and ethically defensible. This study has IRB approval from

both the University of North Carolina (IRB Study No. 14-1933) and Malawi's National Commission for Science and Technology (IRB Study No. RTT/2/20).

In the first stage, two TAs in each district were randomly selected to participate in the evaluation study then for each VC within these four TAs, eligible beneficiary lists were generated following official program targeting procedures. In the second stage, VCs were randomly selected to arrive at a necessary sample size of 3,500 based on power calculations for key program outcomes. The final sample for the study was drawn from 29 VCs and includes 3,531 households, approximately 47 percent of all eligible households from the four TAs. The baseline survey was then administered over several months from June to September 2013 to this sample. Households were not assigned to treatment (T) and control (C) status until the baseline survey concluded in order to maintain objectivity during data collection. Half of the VCs in each TA were randomly assigned to the treatment group and the other half of the VCs was assigned to the delayed-entry control group. The 1,678 treatment households started receiving the cash transfer right away while the 1,853 control households entered program at a later date. This cluster randomization approach both helps to minimize spillover effects and is considered one of the strongest approaches in evaluation literature with elements of random selection and assignment (Malawi SCTP Evaluation Team, 2013; Shadish, Cook, & Campbell, 2002).

The follow-up survey occurred at the end of 2014. The study retained 95 percent of the baseline sample and detailed attrition analysis finds no evidence of selective attrition. At the time of follow-up data collection, treatment households had received five or six cash payments equivalent to about a year's worth of transfers since each accounted for two months (Malawi SCTP Evaluation Team, 2015).

Adolescent Sampling and Surveys

This study uses data from both household and adolescent surveys. The household survey is the main survey instrument and collects information on demographics, food and non-food expenditures, productive activity, education, and health, among others. The Young Person's module was administered to adolescents aged 13-19 (baseline) and 14-22 (follow-up) currently living in the household and collected

information on outcomes such as psychosocial well-being, risky sexual behaviors, substance use, and social support. Up to three adolescents per household were interviewed and the youngest three were prioritized if a household had more than three eligible members. Among the adolescent population within the target age range, approximately 76 percent of the total possible adolescents were interviewed in both waves. Since this represents a select sample within the total household, we include weights to account for the probability of being interviewed. Our analysis uses the sample of adolescents who were interviewed in both waves resulting in a sample size of 1,332 in each wave.

Due to the sensitive nature of the questions, individuals were interviewed in private by same sex interviewers. Informed consent was obtained from parents of adolescents ages 17 and under, and these individuals also gave their assent. For adolescents ages 18 and above, informed consent was obtained directly from the individual.

Randomization and Summary Statistics

Table 4.2. Baseline randomization balance in key program indicators

		Panel	
	Full sample	T	C
Observations	*3,531*	*1,608*	*1,761*
Poverty and Food Security			
Poverty rate, individuals (%)	85.2	90.3	92.8
Eat only one meal per day (%)	19.3	21.3	19.1
Economic Activity			
Operate an enterprise (%)	23.1	24.1	22.6
Cultivate land (%)	95.2	95.6	96.0
Selling any crops (%)	22.7	21.6	23.9
Adult Health (age 50+)			
Morbidity (%)	53.2	55.9	49.9
Any disability (%)	58.6	13.9	14.9
Schooling & Labor			
Ganyu work for pay (age 10-17)	40.8	41.1	38.9
School enrollment (14–17)	70.5	64.3	71.3
Young Child Health & Nutrition			
Underweight (age 0-5)	17.6	19.3	16.9
Consumed Vit A rich foods previous day (6-59 months)	67.0	67.4	60.9

Data source: Table 5.1.1 from Malawi SCTP Baseline Report, 2014

The main objectives of the program are to increase decrease poverty and hunger and increase child human capital. Therefore we tested primary outcomes aligned with these goals including

consumption, food security, economic productivity, schooling, and health and nutrition of young children. Table 4.2 shows summary statistics of some of these primary outcomes at baseline. We tested for statistical differences between the two treatment arms using OLS regression and accounting for the survey design to adjust standard errors. Table 4.2 shows that randomization was successful in all key program outcomes for the panel of households interviewed in both waves as the treatment and comparison groups are balanced across these variables (p-value<0.1).

Table 3. Baseline randomization in outcomes and control variables

	Household Panel	
	T	C
Observations	*1,608*	*1,761*
Household Characteristics		
Per Capita Consumption (mean annual MWK)	43,891	41,357
Household size (mean)	4.5	4.5
Number of shocks in past 12 months (mean)	2.5	2.5
Age of head (mean)	58.7	56.8
Female head (%)	83.2	84.8
Head has chronic illness (%)	47.2	40.5
Head ever attended school (%)	27.8	30.4
	Adolescent Panel (13-19)*	
	T	C
Observations	*676*	*656*
Individual Characteristics, Mediators, and Outcomes		
Male (%)	53.8	50.1
Age (mean)	15.3 (1.8)	15.2 (1.7)
Orphan (under 18) (%)	46.4	43.4
Enrolled in school (%)	68.5	75.3
Ever had sex (%)	31.8	26.8
Illness past 2 weeks (%)	19.6	16.8
Perceived Stress Scale (range of 4-20) (mean)	14.8 (3.2)	15.0 (3.4)
Child investment spending index (range of 1-3) (mean)	1.0 (0.7)	1.0 (0.7)
Any education spending (%)	66.6	68.5
Any health spending (%)	15.8	15.1
Any clothing spending (%)	14.8	18.3
18 and under		
Number of child well-being items (range of 1-3) (mean)	1.5 (0.9)	1.5 (1.0)
Extra set of clothing (%)	82.5	79.6
Shoes (%)	30.1	32.1
Blanket (%)	40.0	41.4
Depressed (%)	43.7	47.5
Depressed male (%)	39.3	45.9
Depressed female (%)	48.9	49.2
CES-D score (mean)	9.6 (5.5)	10.2 (5.5)

*Adolescent panel is the sample of adolescents interviewed in both waves

We also tested for baseline balance amongst all outcome and control variables used in this study including variables that the literature suggests may be correlated with mental health. The top panel of Table 4.3 displays summary data of household-level characteristics at baseline and we find no significant differences between treatment arms (p-value<0.1). The summary data shows that households in our sample tend to have older, female heads and an average size of 4.5 members. In the bottom panel, we display summary data for individual characteristics, mediators, and outcomes and also find that there is good balance between T and C adolescents. Summary baseline data show our adolescent sample has an average age of 15, slightly less than half are orphans, and upwards of two-thirds are enrolled in school.

Estimation Strategy

Specification

With respect to the randomized design of this study, we analyze the average treatment effect (ATE) of the intervention on mental health outcomes using Wave 2 cross-section data. The regression adjusted ATE program impact on mental health is estimated with the following equation:

$$(1) \qquad Y_i = \alpha + \gamma T_i + \beta_1 X_{i2} + \beta_2 X_{i1} + e_i$$

In Equation 1, Y_i is an outcome measure for mental health, T_i is an indicator for treatment status, X_{i2} is the set of individual wave 2 controls while X_{i1} is the set of baseline covariates. We use OLS regression for our CES-D measure and a logit model for our depression indicator and report marginal effects.

To take advantage of the panel data and account for external time trends, we also employ a Differences-in-Differences (DD) model. Equation 2 shows the basic empirical specification where T*P is the DD estimate of the treatment effect, and includes indicators for treatment status (T_i) and second time period (P_t), X_{it} is the set of time varying control variables while X_{i1} is the vector of baseline covariates.

$$(2) \qquad Y_{it} = \alpha + \delta(T_i * P_t) + \gamma T_i + \lambda P_t + \beta_1 X_{it} + \beta_2 X_{i1} + e_{it}$$

The error terms (e_{it}) are clustered at the VC level to account for the clustering at the level randomization to treatment. Probability sampling weights for adolescents are used to make the results representative of the target population in the study area.

In all regressions described above, individual controls include age and sex as well as baseline values of the following variables: being an orphan (for those 18 and under), morbidity in the two weeks prior to the interview, and ever having had sex. These variables were chosen because they are predictive of mental health and thus improve the precision of the impact estimates. We also use controls for other variables that would contribute to parenting behaviors and decision-making including baseline values of the household head's sex, age, and education, as well as household characteristics including household size, total members in different age groups, consumption, and a district dummy for living in Salima.

Mediation

While randomized experiments in social sciences are esteemed for their ability to provide causal estimates of treatment effects, one common criticism is that they do not provide an explanation for why an effect is observed. The identification of causal mechanisms, however, can improve our understanding of these relationships, which may improve policy design. Mediation analysis helps to identify these causal mechanisms (mediators) that lie in the causal pathway between the treatment and the outcome.

In this analysis, we estimate the impact of proposed pathways through the causal steps method proposed by Baron and Kenny (1986). This approach focuses on establishing the necessary conditions for mediation to occur and validates the impact of mediating pathways by testing a sequence of logical relations between X, Y, and M as shown in Figure 1. The equations in Figure 1 are modified for longitudinal analysis from Baron and Kenny's causal steps for mediation analysis (MacKinnon et al., 2002). Each meditational pathway is estimated separately and includes indicators for treatment, time, and the set of X covariates as displayed in Equation 2.

Three conclusions are necessary (Baron & Kenny, 1986):

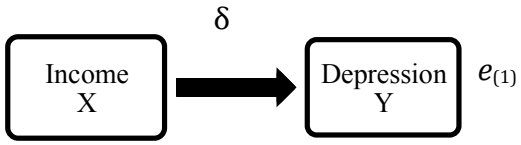
1) Treatment significantly affects the outcome variable (δ)

2) Treatment significantly affects the mediator (γ)

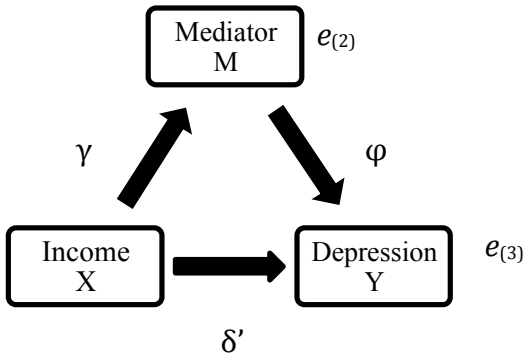
3) When controlling for the mediator in the outcome model, the previous treatment effect is notably diminished with the strongest evidence of mediation occurring when δ' is

significantly indistinguishable from zero

Figure 4.1. Path diagram and equations for mediation



$$Y_{it} = \alpha_{(1)} + \delta(T_i * P_t) + e_{it(1)}$$



$$M_{it} = \alpha_{(2)} + \gamma(T_i * P_t) + e_{it(2)}$$

$$Y_{it} = \alpha_{(3)} + \delta'(T_i * P_t) + \varphi M_{it} + e_{it(3)}$$

The strength of this approach is that in the context of a single randomized experiment it provides evidence that the treatment causes the outcome variable, treatment causes the mediating variable, and that the data are consistent with the proposed mediating variable. On the other hand, the Baron-Kenny causal steps do have some limitations. They do not give a direct estimate of the size of indirect effect of T with standard errors to perform significance tests, nor do they provide a joint test of these three conditions (MacKinnon et al., 2002). In this study, we use the causal steps in order to establish the necessary conditions for mediation with the caveat that we are not precisely identifying the indirect impact. This method is used in a number of similar papers that examine how cash transfers operate through mediating variables (Ozer et al., 2011; Baird et al., 2013; Handa et al., 2015).

Results

In this paper, we keep the panel of adolescents that were interviewed in both rounds. Since this subsample is part of a larger panel of adolescents (those in the household survey at both waves but only interviewed in one round), we show in Appendix Table B2 that our sample panel is almost identical in observed outcome and control variables.

Table 4.4. Wave 2 summary statistics

	T	C
Mental Health Outcomes		
CES-D10 score (mean)	9.2 (5.1)	10.6 (5.4)
Depressed (CES-D10>=10) (%)	43.6	55.2
Depressed male (%)	46.7	49.0
Depressed female (%)	39.9	61.3
Pathways		
Stress scale (range of 4-20) (mean)	12.7 (3.2)	**14.1 (3.0)**
Child investment spending index (range of 1-3) (mean)	1.3 (0.8)	**0.8 (0.7)**
Any education spending (%)	68.6	**53.8**
Any health spending (%)	14.1	**10.0**
Any clothing spending (%)	44.6	**18.4**
(*18 and under)*		
Number of child material items (range of 1-3) (mean)	2.2 (0.9)	**1.8 (1.0)**
Extra set of clothing (%)	94.4	90.8
Shoes (%)	60.9	**46.2**
Blanket (%)	62.6	**46.0**
Enrolled in school (%)	69.2	**55.8**
Illness past 2 weeks (%)	13.5	11.7
Observations	*676*	*656*

Notes: Significant differences between baseline T and C groups in bold (p-value<0.1)

At baseline (Table 4.3), adolescents in beneficiary households had an average CES-D score of 19.6 and 44 percent were above the threshold (CES-D>=10) for displaying depressive symptoms. From here on, we refer to being above the cutoff as 'depression' but still mean symptomatology and not clinical diagnosis. At follow-up in Table 4.4, we find the prevalence of depression for the treatment group is the same, but there is a 7-percentage point (pp) increase in depression in control households (rising from 48 to 55 percent). Separating males and females, the cash transfer appears to be more protective for female mental health over their male counterparts. At follow-up, 43 percent of treatment females are depressed compared to a much large 60 percent of control females. On the other hand, treatment males at follow-up

are more likely to be depressed than at baseline (7 pp increase) and have similar depression rates to than control males (T: 45 %, C: 47 %).

Wave 2 summary results in Table 4.4 also show that parental stress was lower and child investment was higher for the treatment group at follow-up. Parents of the treatment adolescents score an average of 12.7 on the Perceived Stress Scale (range of 4-20) compared to a score of 14.1 for parents of control adolescents. Treatment adolescents are more likely to have had money spent on them at follow-up in each child specific spending category (education, health, and clothing). Moreover, for adolescents 18 and under, the treatment group is more likely to have shoes, an extra set of clothing, and a blanket.

Empirical analysis

First, we run logit regressions on baseline data to identify determinants of depression within our sample. In Table 4.5, Column 1 is the unadjusted impact, Column 2 includes individual covariates, and Column 3 includes individual and household covariates. Consistent with successful randomization, treatment is insignificant in all three regressions. Except for school enrollment in the second model and male in the third model, we find both individual and household characteristics do a poor job of predicting adolescent depression. In the following regressions, we use the full list of baseline individual and household controls to improve precision of our estimates but only display program impacts to provide concise tables.

Table 4.5. Determinants of depression (CES-D>=10) using baseline data

	Full Adolescent Sample		
	(1)	(2)	(3)
Treatment	-0.04	-0.04	-0.04
	(0.04)	(0.04)	(0.04)
Age Dummies			
Age 14		-0.00	0.01
		(0.04)	(0.04)
Age 15		0.03	0.04
		(0.04)	(0.04)
Age 16		0.06	0.08
		(0.06)	(0.06)
Age 17		0.04	0.05
		(0.06)	(0.06)

Age 18		0.03	0.07
		(0.05)	(0.05)
Age 19		0.03	0.05
		(0.07)	(0.07)
Male		-0.06	-0.06
		(0.04)	(0.04)*
Enrolled in school		-0.07	-0.06
		(0.04)*	(0.04)
Ever had sex		-0.02	-0.01
		(0.04)	(0.04)
Past 2 weeks, suffered from illness or injury		0.05	0.04
		(0.05)	(0.05)
Orphan		0.01	0.01
		(0.03)	(0.03)
Head went to school			0.04
			(0.05)
Head can read			-0.08
			(0.06)
Head female			-0.03
			(0.05)
Head age			0.00
			(0.00)
Head widow			-0.01
			(0.03)
Salima			0.05
			(0.05)
Total members 6 to 11			0.01
			(0.02)
Total members 12 to 17			-0.02
			(0.02)
Total members 18 to 64			-0.04
			(0.02)
Total members 65+			-0.04
			(0.03)
Household size			0.01
			(0.02)
Baseline log per capita expenditure			-0.02
			(0.03)
N	1,330	1,324	1,320

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Table 4.6 shows the wave 2 cross-sectional (CS) and differences-in-differences (DD) regression results on mental health using both our indicator for depression (CES-D>=10) and the CES-D score (range 0-30) as dependent variables. Columns 1 and 3 show that using Wave 2 single-differences, the SCTP has a positive impact (negative program effect) on both depression and the CES-D score. The cash transfer reduces the prevalence of depression among treatment adolescents by 11-percentage points and

lowers the average CES-D score by 1.4 points, an impact of 0.25 standard deviations. In Columns 2 and 4, using panel data and a DD specification, we also find that the program reduces depression and CES-D scores but impacts are smaller and no longer significant.

Table 4.6. Impact of Malawi SCTP on binary measure of depression and CES-D score (T effect on Y)

	Depressed (CES-D>=10)		CES-D score	
	CS	DD	CS	DD
	(1)	(2)	(3)	(4)
Treatment	-0.10	-0.07	-1.36	-0.73
	(0.05)**	(0.08)	(0.50)**	(0.82)
N	1,318	2,638	1,318	2,638

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age dummies, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan; Household baseline characteristics (head—female, age, age squared, ever attended school, chronic illness, married) (log per capita expenditure, household size, total age group categories (0-5, 6-11, 12-17, 65+))

Since the cutoff we use to define depression, CES-D>=10, has not been validated amongst adolescents in Malawi we also explore additional cutoffs at 8, 9, 11, and 12. We repeated the DD analysis presented in Table 4.6 using these other threshold scores (results shown in Appendix Table B3). For each cutoff score, the program impact is similar in size and insignificant. We therefore conclude that the program impacts presented in Table 6 are robust to our choice threshold used to define our binary indicator of depression.

We also ran additional subgroup analysis for characteristics (gender, living in the poorest households, and orphan status) that we argue could make adolescents more vulnerable to mental health problems or to the impact of an income shock to the household.

Table 4.7. Impact of Malawi SCTP on depression by subgroups (T effect on Y)

	Male		Female		Poorest 50% baseline		Orphans	
	CS	DD	CS	DD	CS	DD	CS	DD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.03	0.06	-0.20	-0.21	-0.09	-0.03	-0.16	-0.14
	(0.05)	(0.07)	(0.07)***	(0.12)*	(0.06)	(0.09)	(0.04)***	(0.08)*
N	698	1,398	620	1,240	768	1,539	477	1,049

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age dummies, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan / Household baseline characteristics (head—female, age, age squared, ever attended school, chronic illness, married) (log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

The CS and DD results in Table 4.7 show that the SCTP has differential impacts on each subsample and appears to be primarily protective for females and orphans. From the CS results in Columns 3 and 7, we find that female adolescents in the treatment group are 20 percentage points less likely to be depressed while orphans are 16 percentage points less likely, both significant at the 1 percent level. Compared to the CS results, the DD estimates in Columns 4 and 8 are similar in magnitude for females and orphans respectively but less significant (p-value<0.1).

Mediation

Earlier, we discussed the role of income to impact adolescent mental health and the potential mechanisms that a cash transfer program might work through to affect adolescent mental health. We identified two main parental channels—increased investment in adolescents and reductions in parental stress. We operationalize these channels with the PSS score for parental stress and an index measures for investment that was detailed earlier in the description of our measures. We also identified three individual-level channels including material well-being, schooling, and improved physical health. Measures for these channels include indicators for personal consumption items (extra set of clothes, shoes, and a blanket), spending categories (education, clothing, and health), school enrollment, highest grade achieved, and morbidity in the past 2 weeks.

For each mediator, we first test the causal impact of the cash transfer (T) on the indicator or index measure (M) using the mediator regression defined in the Methods section. In Table 4.8, we show the effect of T on M for both females and orphans since the program significantly impacted these two groups. We also include the full sample in Table 4.8 in order to compare the treatment impacts for these subgroups and address how the cash might have affected these groups differently. In the ensuing analysis, we leave out the full sample because the first condition of causal mediation analysis (T significantly impacts Y) was not met.

Table 4.8. Impact of Malawi SCTP on mediators using DD (T effect on M)

	Full Sample	Females	Orphans
Stress Scale (4-20)	-1.13	-1.14	-1.53
	(0.62)*	(0.62)*	(0.80)*
Number of items (1-3)	0.35	0.22	0.34
	(0.15)**	(0.16)	(0.19)*
Extra set clothes	0.01	-0.04	0.01
	(0.05)	(0.06)	(0.06)
Shoes	0.17	0.11	0.16
	(0.05)***	(0.07)	(0.09)*
Blanket	0.18	0.17	0.18
	(0.07)**	(0.07)**	(0.08)**
Investment (1-3)	0.49	0.37	0.60
	(0.09)***	(0.10)***	(0.10)***
Any education expenditure	0.16	0.12	0.22
	(0.04)***	(0.04)**	(0.05)***
Any health expenditure	0.04	-0.02	0.06
	(0.03)	(0.05)	(0.05)
Any child clothing expenditure	0.29	0.27	0.32
	(0.05)***	(0.06)***	(0.06)***
Enrolled	0.19	0.15	0.25
	(0.03)***	(0.05)***	(0.05)***
Highest grade achieved	0.30	0.19	0.68
	(0.16)*	(0.22)	(0.20)***
Illness past 2 weeks	-0.01	-0.07	-0.04
	(0.03)	(0.04)	(0.04)
N	2,643	1,242	1.051

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan

Baseline controls: Head characteristics (female, age, age squared, ever attended school, chronic illness, married;)

Household characteristics (Baseline values of log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Results in Table 4.8 indicate that the cash transfer has strong impacts on many of these meditation channels across each sample. For one, parental stress was significantly lowered for each sample by over a point (around 0.3 SD) on the PSS scale (p-value<0.1). The strongest results of treatment are on the investment channels and schooling. The program results in an increase in adolescent ownership of blankets, spending on child clothing and education, investment (number of child specific spending items), and school enrollment. We find that impacts of the cash transfer are moderated in part by the female and orphan samples. In general, we observe smaller impacts for females but larger ones for orphans compared to the full sample. For example, school enrollment impacts are significant and sizeable

for all groups, but females are less likely to be enrolled than the full sample (15 pp. vs. 19 pp. respectively), while orphans are 25 percentage points more likely to be enrolled. Additionally, the impact on the investment index, which sums spending items on child clothing, education and health care, is smallest for females and largest for orphans. In contrast, the cash transfer does not affect adolescent ownership of an extra set of clothes, health spending, or whether adolescents were ill in the past 2 weeks. These findings indicate that the program had a strong effect on channels that might influence mental health among adolescents. Furthermore, the observed effects are in the expected directions: the cash transfer improves parental stress, increases spending on adolescents, ownership of personal items, and school enrollment.

Finally, to estimate the overall mediation effect of these channels (T effect on Y through M), we follow the steps for causal mediation and re-estimate the regressions presented in Columns 4 and 8 in Table 4.7, but this time also include the values of potential mediation channels. To satisfy the second condition of causal mediation, we continue analysis only for those mediators that were significantly impacted by treatment in both groups. Consequently, each channel except physical health is represented in the measures that we analyze for a mediation impact: Parental Stress Scale, the investment index, material well-being index, and school enrollment. Although blanket ownership and spending on clothing and education are significant, they are encompassed within the indexes.

In order to justify casual mediation we must also uphold sequential ignorability, which consists of two assumptions: (1) treatment must be independent of both potential values of outcome and mediating variables (2) the mediator is independent of all potential values of the outcome conditioned on the observed treatment and pretreatment covariates (Imani, Keele, and Tingely, 2010). The first part of sequential ignorability is effectively taken care of with randomization to treatment, but the second part implies that mediators must be regarded as "as-if" randomized among treatment arms (Keele et al., 2015). To satisfy this second part of this assumption, we must control for all pretreatment covariates that may confound the relationship between the mediators and mental health so that the outcome is modeled as a

function of the mediator, treatment, and pretreatment covariates. Our rich baseline data allows us to control for pretreatment covariates we believe to impact mental health and mediators, but also allows us to condition on the pretreatment levels of each mediator. Each model in Table 4.9 includes baseline values of the mediator tested in the model and the covariates that may lie on the causal pathway.

Assuming that the true relationship between these channels and depression is modeled correctly, our mediators pick up the indirect effects of the cash, while the effects remaining in the treatment dummies are the direct effects of the cash transfer. Table 4.9 presents the results for each of the main channels separately and the Column 5 and 10 show the results for all channels together.

A similar pattern emerges for females and orphans in Table 4.9. The size of the treatment effect on depression is robust across all mediation measures, but for most models, it is no longer significant, indicating slight mediation. For a couple of channels though, parental stress (Columns 1 and 6) and investment for orphans (Column 8), there is no meditation since the treatment effect is both unaffected and significant. Other results indicate that school enrollment is an important protective channel of mental health as the impact on enrollment in Columns 8 and 12 is negative and significant. The effect is particularly strong for females; enrollment reduces depression prevalence by 17 pp (p-value<0.01).

Finally, we include all mediators as regressors in Columns 5 and 10. There is little combined effect of these channels on depression. For orphans, the treatment effect is mediated to a greater extent than in any single mediation model, but for females, the effect size is unchanged. Nevertheless, school enrollment is still a strongly protective channel for both females and orphans depression.

Table 4.9. Impact of cash transfer with the inclusion of mediators (DD) (T effect on Y accounting for M)

	Females					Orphans				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	-0.22	-0.20	-0.20	-0.19	-0.19	-0.13	-0.12	-0.13	-0.12	-0.08
	(0.12)*	(0.12)	(0.12)	(0.12)	(0.13)	(0.08)*	(0.07)	(0.08)*	(0.07)	(0.08)
Stress scale	-0.00				-0.00	0.01				0.01
	(0.01)				(0.01)	(0.01)				(0.01)
Number of personal items		-0.01			-0.01		-0.04			-0.04
		(0.03)			(0.02)		(0.03)			(0.03)
Investment			-0.04		0.00			-0.02		0.00
			(0.03)		(0.03)			(0.03)		(0.03)
Currently enrolled in school				-0.17	-0.16				-0.08	-0.09
				(0.05)***	(0.07)**				(0.05)*	(0.05)*
N	1,240	1,093	1,240	1,239	1,092	1,049	1,034	1,049	1,049	1,034

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.

Individual controls: age, male, baseline: enrolled, ever had sex, morbidity past 2 weeks, orphan

Baseline controls: Head characteristics (female, age, age squared, ever attended school, chronic illness, married;)

Household characteristics (Baseline values of log per capita expenditure, household size, total age group categories,(0-5, 6-11, 12-17, 65+))

Discussion

This study uses an unconditional cash transfer experiment to assess the effects of a positive income shock on adolescent mental health in beneficiary households. Our main findings indicate that a large-scale social cash transfer program may not have the capacity to reduce depression of all adolescents living in beneficiary households but may benefit some of the most disadvantaged groups, in this case, females and orphans who are generally at greater risks of mental disorders during adolescence.

In our analysis, we use longitudinal data from adolescents ages 13-22 and examine the effect an unconditional cash transfer from Malawi's SCTP has on mental health outcomes by comparing adolescents living in treatment households to those in control households. Results from our differences-in-differences analysis show that for the full sample of adolescents, the Malawi SCTP did not result in a significant reduction in the

likelihood of depression (CES-D score 10 or higher). Further heterogeneity analysis indicates that the program does, however, result in a significant 22-percentage point decline in depression amongst females and a 14-percentage point decline for orphans (Table 4.7). The impact for females is comparable to the decline in psychological distress experienced by adolescent girls receiving a randomized cash transfer in Zomba, Malawi (Baird et al., 2013). In contrast though, results from the Kenyan CT-OVC study were driven by the decline in male depression using the same CES-D instrument (Kilburn et al., 2015). On the other hand, that study did find stronger effects for orphans over non-orphans. The main advantage we have over these previous studies is a stronger identification strategy that uses longitudinal data and a DD specification to control for baseline mental health and unobserved trends across waves. Our results show that with single-differences, the SCTP significantly reduced depression by 10 percentage points for the full sample but this result overestimates the program impact if we account for baseline levels of depression.

The second part of our analysis examined potential mediation channels in order to better explain how income increases could improve adolescent mental health. We find that the SCTP had strong impacts on a number of channels we proposed might explain the relationship between a cash transfer program and decreased mental health. Results show that treatment led to reductions in parental stress, increases in child investment spending and material well-being, and a large increase in school enrollment. Although the SCTP had large effects on these potential intervening channels, causal mediation analysis shows that these pathways do not mediate much of the direct path from the cash transfer to adolescent mental health. Nevertheless, school enrollment has an important protective effect and leads to a significant reduction in female depression (-17 pp) and orphan depression (-8 pp). The protective impact of schooling for adolescents is common finding in many cash transfer studies, especially for females as school enrollment contributes to delayed sexual debut (Handa et al., 2014), declines in early pregnancy (Handa et al., 2015; Baird et al., 2010), and even reduction in sexually transmitted disease (Baird et al., 2012).

Limitations

There are a few limitations of our study that merit discussion. First, it is conceivable that adolescents in beneficiary households felt pressured to answer the CES-D questions more optimistically than did the control group with the implication that our self-reported mental health measures are biased. However, since we only found significant differences for particular subgroups, this would have be the case only for females and orphans. Summary results even show that in relation to baseline depression, males in beneficiary households were more likely to be depressed (Table 4.3 and 4.4).

Second, there are inherent estimation challenges in identifying the pathways from income to child development outcomes (Strauss & Thomas, 2008; Shadish et al., 2002). For one, some of our constructs such as stress and investment are latent variables for the true underlying behaviors and are not externally manipulated meaning the model may lack some predictive power (Bullock & Ha, 2011). Additionally, since income from cash transfer programs is used to improve many aspects of beneficiaries' lives at the same time, our study design is not ideal to identify causal mechanisms underlying the observed impacts. Nevertheless, the randomization of economic conditions, the use of longitudinal data, and the set of rich baseline covariates that we use to control for confounding, means that we can reasonably make causal predictions for whether these observed channels are mediating the cash transfer (Strauss & Thomas, 2008).

A final limitation of the present study is that the amount of time may have been too short for cash transfers to impact depression. While we do not find that tested pathways explain much of the treatment effect, the SCTP still has strong impacts on these channels that may over time continue to improve adolescent well-being including their mental health.

Conclusion

This paper provides new evidence from the Malawi Social Cash Transfer Program, a national unconditional cash transfer program in Africa, on the whether household-level income increases affect adolescent mental health. Given the combined public health and long-term developmental consequences

of mental health problems during adolescence, especially in low-income settings, policies should aim to better protect adolescent mental health. Overall, the results presented in this paper do not provide strong evidence that a positive income shock in the form of an unconditional cash transfer can improve adolescent mental health for all young people in beneficiary households, at least within a year's time. Nevertheless, it does provide evidence the program had significant protective effects on certain vulnerable groups. Although improving adolescent mental health is not a specific objective of these types of cash transfer programs, if it can reach those most vulnerable to mental health disorders, these programs may have important public health ramifications. Therefore, policymakers should be aware of potential of heterogeneous program effects when designing interventions to reach adolescents.

The results from this study contribute to emerging evidence on the influence of social cash transfer programs in SSA to promote adolescent development and safe transitions to adulthood by targeting household poverty. However, there is still more research needed to explain why these interventions affect adolescent development since pathways that we explore provide little evidence of mediation. Schooling appears to be the most promising pathway through which cash transfers may improve adolescent mental health and contribute to the successful transition to adulthood. Given the positive impacts that SCTs have had on female development outcomes including schooling, mental health, and sexual behaviors, it would be interesting to further investigate these inter-relationships to confirm causal pathways. Moreover, future researchers would be wise to consider the effects of social cash transfers over longer time frames both to assess if improvements in mental health occur after a longer time frame and to discover whether effects are sustained into adulthood.

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APPENDIX A: ADDITIONAL TABLES FROM CHAPTER 2

Appendix Table A1. Summary of impacts in program objective areas

Objective Area	All households	Poorest 50 percent of households
Consumption, food security		
Consumption	**	**
Food consumption	**	**
Meals per day	**	**
Poverty	**	N/A
Poverty gap		**
Squared poverty gap		**
Economic productivity		
Livestock	**	**
Crop production	**	**
Agricultural assets	**	**
Non-agricultural assets	**	**
Health, nutrition of young children		**
Weight-for-age		**
Weight-for-height		
Height-for-age	**	
3+ meals per day		
Illness		
Curative care		
Schooling, child work, material needs	**	**
Enrolment ages 6-13	**	**
Enrolment ages 14-17	**	**
Hours unpaid work	**	**
Hours paid work		
Material needs (blanket, clothes, shoes)		
Safe transition to adulthood (13-19-year-olds)		**
Sexual debut	**	**
Early pregnancy		
Mental health		
Health	**	**
Chronic illness	**	**
Morbidity	**	**
Curative care		
Caregiver Stress		

Source: Malawi Social Cash Transfer Program Midline Impact Evaluation Report (2015)
(**) denotes statistically significant in the hypothesized

APPENDIX B: ADDITIONAL TABLES FROM CHAPTER 4

Appendix Table B1. CES-D short form questions

1. I was bothered by things that usually don't bother me.
2. I had trouble keeping my mind on what I was doing
3. I felt depressed.
4. I felt that everything I did was an effort.
5. I felt hopeful about the future.
6. I felt fearful.
7. My sleep was restless.
8. I was happy.
9. I felt lonely.
10. I could not "get going."

Appendix Table B2. Comparison of two adolescent panels: summary statistics at Wave 2

	Adolescent panel, in household both waves	Adolescent panel, interviewed both waves
Age	16.4 (2.0)	16.4 (1.8)
Male	53.2	53.1
Enrolled	63.6	63.2
Ever had sex	38.2	38.8
Illness past 2 weeks	12.4	12.8
Orphan	44.7	43.7
CES-D10 score	19.6 (5.3)	19.8 (5.3)
Depressed	49.3	48.6
Observations	*1958*	*1332*

Appendix Table B3: Impact of cash transfer using different CES-D thresholds to define depression

	CES-D cutoff >=				
	8	9	10	11	12
Treatment	-0.02	-0.05	-0.07	-0.07	-0.07
	(0.07)	(0.08)	(0.08)	(0.07)	(0.06)
N	2,638	2,638	2,638	2,638	2,638

Notes: Standard errors in parenthesis clustered at the VC level, *p<0.1; ** *p*<0.05 ***p<0.01.